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MDCCLXXIX.

for: Banks

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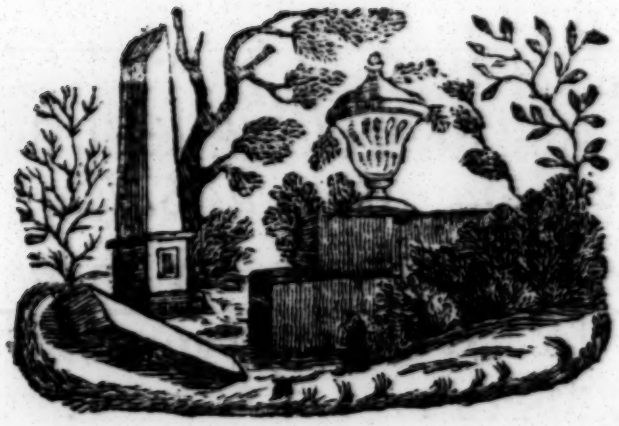
ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.



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PHILOSOPHICAL TRANSACTIONS.

I. *An Account of a Cure of the St. Vitus's Dance by Electricity. In a Letter from* Anthony Fothergill, *M. D. F. R. S. at* Northampton, *to* William Henly, *F. R. S. Communicated by Mr.* Henly.

Northampton,
Oct. 28, 1778.

SIR,

Read Nov. 5, 1778.

AGREEABLE to my promiſe, I now proceed to give you ſome account of a recent cure performed by electricity, which will, I think, afford you much pleaſure.

ANN AGUTTER, a girl of ten years of age, of a pale, emaciated habit, was admitted an out-patient at the Northampton Hoſpital on the 6th of June laſt. From her father's account it appeared (for ſhe was ſpeechleſs and with difficulty ſupported from falling by two aſſiſtants) that ſhe had for ſix weeks laboured under violent convulſive motions, which affected the whole frame, from which ſhe had very ſhort intermiſſions, except during ſleep; that the diſeaſe had not only impaired her memory and intellectual faculties, but of late had deprived her of the uſe of ſpeech.

Volatile and fetid medicines were now recommended, and the warm bath every other night; but with no better ſucceſs, except that the nights which had been reſtleſs became ſomewhat more compoſed. Bliſters and anti-ſpaſmodics were directed, and particularly the flowers of zinc, which were continued till the beginning of July, but without the leaſt abatement of the ſymptoms; when her father growing impatient of fruitleſs attendance at the hoſpital, I recommended, as a dernier reſort, a trial of electricity, under the management of the Rev. Mr. UNDERWOOD, an ingenious electrician. After this I heard no more of her till the firſt of Auguſt, when her father came to inform me that his daughter was well, and deſired ſhe might have her diſcharge. To

which, after expreſſing my doubts of the cure, I conſented; but ſhould not have been perfectly convinced of it, had I not received afterwards a full confirmation of it from Mr. UNDERWOOD, dated Sept. 16, an extract from whoſe letter I will now give you in his own words.

" I have long expected the pleaſure of ſeeing you, " that I might inform you how I proceeded in the cure " of the poor girl. As the caſe was particular, I have " been very minute, and wiſh you may find ſomething " in it that may be uſeful to others. If you think it pro- " per, I beg you will ſtate the caſe medically, and make " it as public as you pleaſe.

" July 5. On the glaſs-footed ſtool for thirty minutes: " ſparks were drawn from the arms, neck, and head, " which cauſed a conſiderable perſpiration, and a raſh " appeared in her forehead. She then received ſhocks " through her hands, arms, breaſts, and back; and from " this time the ſymptoms abated, her arms beginning to " recover their uſes[a].

" July 13. On the glaſs-footed ſtool forty-five mi- " nutes: received ſtrong ſhocks through her legs and " feet, which from that time began to recover their " wonted uſes; alſo four ſtrong ſhocks through the jaws, " ſoon after which her ſpeech returned.

(a) The coated bottle held near a quart.

" July 23. On the glaſs-footed ſtool for the ſpace of " one hour: ſparks were drawn from her arms, legs, " head, and breaſt, which for the firſt time ſhe very ſen- " ſibly felt; alſo two ſhocks through the ſpine. She " could now walk alone; her countenance became more " florid, and all her faculties ſeemed wonderfully " ſtrengthened, and from this time ſhe continued mend- " ing to a ſtate of perfect health.

" Every time ſhe was electrified poſitively, her pulſe " quickened to a great degree, and an eruption, much " like the itch, appeared in all her joints."

Thus far Mr. UNDERWOOD. To complete the hiſtory of this ſingular caſe, I this day (Oct. 28) rode ſeveral miles, on my return from the country, to viſit her; and had the ſatisfaction to find her in good health, and the above account verified in every particular, with this addition, that at the beginning of the diſeaſe ſhe had but ſlight twitchings, attended with running, ſtaggering, and a variety of involuntary geſticulations which diſtinguiſh the St. Vitus's dance, and that theſe ſymptoms were afterwards ſucceeded by convulſions, which rendered it difficult for two aſſiſtants to keep her in bed, and which ſoon deprived her of ſpeech and the uſe of her limbs. The eruptions which appeared on the parts electrified ſoon receded, without producing any return of the

ſymptoms,

ſymptoms, and therefore could not be called critical, but merely the effect of the electrical ſtimulus. Having given her parents ſome general directions as to her regimen, &c. I took my leave, with a ſtrong injunction to make me acquainted in caſe ſhe ſhould happen to relapſe. Before I conclude, it may not be improper to obſerve, that ſome time ago I was fortunate enough to cure a boy who had long had the St. Vitus's dance (though in a much leſs degree) by electricity. A violent convulſive diſeaſe, ſomewhat ſimilar to the above, though, if I recollect right, not attended with the *aphonia*, was ſucceſsfully treated in the ſame way by Dr. WATSON, and is recorded in the Philoſophical Tranſactions. May we not then conclude, that theſe facts alone, and more might perhaps be produced, are ſufficient to intitle electricity to a diſtinguiſhed place in the claſs of antiſpaſmodics?

I am, &c.

II. *A Case in which the Head of the* Os Humeri *was sawn off, and yet the Motion of the Limb preserved.* *By Mr.* Daniel Orred, *of* Chester, *Surgeon.* *Communicated by* Thomas Percival, *M. D. F. R. S.* and *A. S. and Member of the Royal Society of Physicians at* Paris.

Manchester,
Sept. 20, 1778.

SIR,

Read Oct. 12, 1778.

A VERY eminent surgeon at Chester has desired me to transmit the inclosed case to the Royal Society, and I hope it will be deemed worthy of publication. It not only affords a confirmation of an important fact inserted in the Philosophical Transactions, vol. LIX. art. VI.; but shews also that the chirurgical improvement, proposed in that article by my ingenious friend Mr. CHARLES WHITE, may be extended to operations on other parts of the human body.

I am, &c.

THO. PERCIVAL.

A FRIEND

A FRIEND of mine, an ingenious ſurgeon, ſettled at Tarporley, in this county, ſent for me about the middle of laſt month to ſee a patient of his, a gentleman's ſervant in that neighbourhood. This was a man of about forty years of age, who had much injured a good conſtitution by hard drinking before the following accident happened to him.

From an injury received more than three years before the time I ſaw him, by a fall from the top of a ladder, I found a ſuppuration had taken place in the ſhoulder joint. The matter had made its way through three ſmall openings; one in the *axilla*, directly oppoſite to the cavity of the joint; the other two lower than the ſtrong tendons of the pectoral muſcle, and betwixt the *deltoides* and *biceps* muſcles. Upon introducing a probe into the joint by the upper orifice, I found the head of the *os humeri* exceedingly carious. A few weeks before I ſaw him, a collection of matter had formed upon his foot, I ſuppoſe from an abſorption and tranſlation from the ſhoulder. Upon letting out the matter with a lancet, I found the metatarſal bones alſo very carious: with theſe ſhocking complaints no wonder he was much enfeebled and reduced. As the diſeaſe in his ſhoulder would evidently ſoon have put a period to his life without immediate relief, I propoſed to him,

him, either to amputate the arm at the diſeaſed joint, or, with a view of making it of ſome uſe to him, endeavour to ſaw off the head of the affected bone only. As the leaſt of two evils he choſe the laſt; though this indeed is a moſt painful, hazardous operation. We are indebted to Mr. WHITE, of Mancheſter, for the mode of this operation, as well as for many other valuable hints and diſcoveries in ſurgery. In order to allow the arm as much action as poſſible after the operation, I began my inciſion a little above the joint, and continued it in a right line directly through the middle of the fleſhy portions of the *deltoides*, and a little lower than its inſertion: then elevating the arm to relax the muſcle, an aſſiſtant with both hands diſtended the upper part of the opening made by the inciſion, whilſt, with a narrow knife, I endeavoured, by the direction of the fore-finger of the left hand, carefully to divide the capſular ligament. This was effected with very great difficulty, as from preceding inflammation it was much thickened, and adhered cloſely to the joint: and till it was ſeparated nearly round (for the under, and moſt dangerous part to cut through, was corroded with matter) I found my utmoſt efforts to throw the head of the bone out of its ſocket quite ineffectual. After a ſufficient ſeparation I made the diſlocation, by preſſing the elbow to the body with one hand, and with the

the other pulling the head of the *humerus* directly towards me. After guarding the great artery againſt the action of the ſaw, by introducing a piece of paſte-board under the bone, I ſeparated it acroſs, as Mr. WHITE directs, as low down as I poſſibly could to prevent an exfoliation. The loſs of blood was very trifling. After dreſſing the wound very ſuperficially, I took particular care that the artery was not preſſed upon by the bandages; and adviſed, when the inflammation ſubſided, and a good digeſtion came on, that his arm ſhould always be dreſſed when the body was erect, and ſuſpended a little from it, with the fore arm a little bent: this was accordingly done. In a few days after the operation, he got up, and continued to ſit up the day through ever after. He had a cold infuſion of the Peruvian bark with the weak ſpirit of vitriol ordered him. In conſequence of his very reduced habit of body, his ſhoulder was long in curing. A ſmall exfoliation took place. The cure, I fancy, was alſo much retarded by the diſeaſed foot, which ſtill continued very bad; the man being ſo exceedingly terrified by the former operation, that he would not ſuffer us to do any thing to any purpoſe to it.

I ſaw him about three months after the accident. The wound was nearly cicatrized; but the oſſification was not ſo far advanced as I expected it would have been, the

callus being much ſmaller than the lower part of the *humerus*, and bending with the weight of the arm. However, he could raiſe it from his body more than could be well ſuppoſed, and had the perfect flexure and uſe of his fore arm. This caſe, with all the diſagreeable circumſtances attending it, ſtrongly proves the utility of the above operation. By a ſimilar operation diſeaſes of other joints may be as eaſily cured.

About five years ago, a young man of the name of MOORES, about ſixteen years of age, and ſon to a farmer at Alderſey, a village in this neighbourhood, applied to me with a diſeaſe in the lower part of the *ulna*, where it joins the bones of the *carpus*. A variety of means for ſeveral years had been tried to no purpoſe, to relieve him before he came to me. The patient was of a good habit of body, and ſeemed to enjoy tolerable good health. With his conſent I carefully ſeparated from the adjoining parts, and ſawed off, more than three inches of the enlarged bone. A *callus* at a proper time formed in the intermediate ſpace. He is now able to undergo the moſt laborious parts of huſbandry buſineſs; though that part of the fore arm where the operation was performed is ſtill ſomething ſmaller than the ſame part in the other arm.

III. *Experiments on ſome Mineral Subſtances.* *By* Peter Woulfe, *F. R. S. Communicated at the Deſire of* William Hunter, *F. R. S. and Phyſician extraordinary to the Queen.*

Read Nov. 19, 1778.

On cryſtal, quartz, and flint.

MONS. BEAUMÉ, a member of the Royal Academy of Sciences or Paris, has aſſerted, that he obtained allum from cryſtal, quartz, and flint. His method to obtain allum from theſe ſubſtances was to make a *liquor ſilicum*, by melting them in a crucible with fixed alkaly, and letting the mixture run *per deliquium*; any acid added to this *deliquium* precipitates the cryſtal, quartz, or flint, which, by means of acid of vitriol, he diſſolved, and obtained allum. I have often repeated this operation on all theſe ſubſtances, but could never obtain a ſingle grain of allum: in lieu thereof I got a ſelenite, and was on that account convinced, that the baſis of theſe ſubſtances was a calcareous earth. Mr. BEAUMÉ's miſtake was, I am certain, owing to the Paris crucibles which he

made uſe of, and which, for want of being well burnt, retained yet their clayey nature; conſequently a portion, and that not a ſmall one, of the clay, is diſſolved by the alkaly; allum muſt therefore in his manner be obtained, as clay is the matrix, or at leaſt contains the earth of allum. It is rather a tedious operation to make the *liquor ſilicum*, and eſpecially in any quantity; for the mixture is very ſubject to froth and boil up in the crucible, and the operation is not compleat until this boiling up ceaſes, and the mixture melts thin. Hence it is natural to conclude, that a large portion of the clay of a Paris crucible is diſſolved by the alkaly during this long operation.

In order to diſcover whether the cryſtal, quartz, or flint, contained any acid, I put in practice the method which I publiſhed in my laſt paper on mineral ſubſtances, to diſcover the acids of horn ſilver and horn mercury; but I could not find they contained any.

On the change of cryſtal into ſelenitical ſpar.

The cryſtal on which I tried the following experiments, and which is partly changed into ſelenitical ſpar, is to be found in that moſt excellent muſeum collected by Dr. HUNTER. This cryſtal comes from near Freyberg in Saxony, and is called in German *nieren-formiger cry-*

ſtal (kidney-formed cryſtal). It is in the form of an uneven cruſt, with ſmall protuberances; a part of it retains all the appearances of cryſtal, but the reſt is opaque, ſomewhat of an aſh colour, and is eaſily powdered: the underpart retains marks of *galena* or potter's lead ore, and in ſome ſpecimens the greateſt part is coated with it.

I took three drams of the opaque part of this cryſtal finely powdered, and mixed it with an equal quantity of fixed alkaly of cream of tartar, and in order to facilitate the union, the mixture was made into a paſte with diſtilled water, then dried and calcined for an hour as in my experiments on horn ſilver (ſee Phil. Tranſ. vol. LXVI. p. 604.). The mixture was then taken out of the phial powdered, and, by digeſtion with diſtilled water at three different times, deprived of its ſaline part: theſe three ſolutions were mixed together, ſaturated with diſtilled vinegar, evaporated to dryneſs and waſhed well with rectified ſpirit of wine. By this means ℥j. and gr. vi. of tartar of vitriol ~~was~~ *were* obtained. The undiſſolved part, remaining after digeſtion with water, when dried weighed ℥ ij. and gr. xxiv.; this was mixed and digeſted with pure acid of nitre, and effervеſced ſtrongly. After this digeſtion the undiſſolved part here remaining, being well waſhed and dried, weighed ℥j. and gr. xv. which I take to be cryſtal, for no ſaline matter was obtained by treating

it

it with acid of vitriol; and hence it appears, that nearly two-thirds of the cryſtal is changed into ſelenitical ſpar. The ſolution which was obtained by the acid of nitre mixed with acid of vitriol, but eſpecially with a ſolution of tartar of vitriol, precipitates copiouſly, and forms a ſelenite. This evidently proves the exiſtence of the calcareous earth, and the tartar of vitriol mentioned above, that of the acid of vitriol in this altered cryſtal.

The unaltered part of this cryſtal, treated in the ſame manner as the altered part, afforded no marks of ſelenitical ſpar.

The change of this cryſtal is, I ſuſpect, owing to the lead ore which adheres to it; the ſulphur of the lead ore furniſhing the acid of vitriol, and the cryſtal the calcareous earth.

I have in the above manner treated a variety of the heavy ſpars, commonly called ſelenitical or gypſeous ſpars, and found them all, as I firſt judged by their appearance and great weight (without being once miſtaken) to conſiſt of acid of vitriol, with calcareous earth and ſome clayey matter. In ſome, the calcareous earth was combined with a larger portion of this acid than in others; and in ſome, the clayey matter was predominant. Among theſe were the following, ſome of which were not before taken for ſelenitical ſpars.

The Derbyſhire and Eckton cauk, which is commonly covered with copper marcaſſite.

Ditto from Saxony, which is found growing like knobs or protuberances on the yellow phoſphoric ſpars.

The *ſpathum ericæ-forme* of Woodward from Derbyſhire.

The ſtellated ſpar, which is found on the *ludus helmontii* from the iſland of Sheppy.

The ſpar from Saxony, reſembling lead ore, and which by many is ſuppoſed to be one, called in German *ſtangen ſpath*. This ſpar is by Baron BORN claſſed with the baſaltes; he has no doubt taken it for a ſhirl, which is claſſed with the baſaltes, but I never found them to have any affinity.

Another ſpar from Saxony, called alſo *ſtangen ſpath*, compoſed of large interwoven priſms, partly red and partly white, and ſemi-tranſparent.

Compact ſemi-tranſparent Auvergne ſpar, ſaid by M. MONET to contain ſulphur; but I could never find an atom of it in this or in any of the other ſpars I tried, and ſuppoſe his miſtake to be owing to a portion of charcoal, flame of charcoal, or duſt, that got into the crucible. Native ſulphur is frequently found growing with calcareous ſpar and gypſum, but it ſhould be conſidered as heterogeneous.

Flaky milk-white ſpar, found with Cobalt and copper ores from Saalfeld.

Two

Two ſpars from the Hartz, compoſed of flakes and cryſtallized.

Ditto from Saxony.

Striated ſpar from Scotland, brought here by Mr. Jeans ~~GOAN~~.

Spar intermixed with cinnabar and ochre of iron from Obermoſchel in the dutchy of Deux Ponts.

Red ochry compact ſpar from Derbyſhire.

I ſhall give a few of my experiments on theſe ſubſtances compared with the common Paris montmartre plaſter ſtone; all theſe were deprived of their humidity before they were ſubmitted to trial.

Two drams of the Paris plaſter ſtone, treated as in the former experiment, with an equal quantity of alkaly of tartar, produced ℥ jſſ. and gr. viii. of tartar of vitriol[a].

The inſoluble part remaining after being deprived of its ſaline part by water, was digeſted with diſtilled vinegar, which effervefced and diſſolved the whole of it, except a ſmall portion, which, when waſhed and dried, weighed gr. vi. and had the appearance of tobacco-pipe clay. Had I taken a pure white tranſparent gypſum, I believe that no ſediment would have been left. From

(a) The tartar of vitriol, of all the trials I make mention of in this paper, was diſſolved in diſtilled water, and mixed with a ſolution of calcareous earth in acid of ſalt, which cauſed a copious ſelenitical precipitation, a clear demonſtration of the exiſtence of the acid of vitriol.

this

this experiment it appears, that gr. xcviii. of vitriol was produced by gr. cxiv. of pure gypſum, for the gr. vi. of clayey matter muſt be deducted.

The like quantity of Derbyſhire cauk, treated in the ſame manner, produced gr. lix. of tartar of vitriol, and the clayey matter, which exactly reſembled the former, weighed gr. xxiii.

The ſtellated ſpar from the iſland of Sheppy, ſubmitted in the ſame proportion to the ſame trials as the foregoing, produced gr. lviii. of tartar of vitriol, and gr. xxii. of clayey matter, which was whiter than in the former.

WOODWARD'S *erica* formed ſpar in the like proportion and manner as the former, afforded gr. xxxviii. of tartar of vitriol, and gr. xlii. of clayey matter.

To ſhew that the calcareous earth in the gypſum contained a greater portion of acid of vitriol than that in the ſelenitical ſpars, I reaſoned thus, having previouſly ſubtracted the clayey matter of each.

If 114 grains of plaſter (the 6 grains of clay being deducted) afford 98 grains of tartar of vitriol, how much ſhould 97 grains of the gypſeous matter contained in cauk afford (for the clayey ſubſtance muſt alſo here be ſubtracted). From this rule it ſhould produce $83\frac{12}{57}$ grains of tartar of vitriol, and nevertheleſs the quantity

was only 59 grains. Hence it is clear, that the calcareous earth of the gypſum contains a greater proportion of acid of vitriol than that of the cauk.

According to this rule, the ſtellated ſpar from Sheppy ſhould have afforded $84\frac{14}{57}$ grains of tartar of vitriol, but it produced only 58 grains.

The *erica* formed ſpar by the ſame rule, ſhould have afforded $65\frac{7}{57}$ grains of tartar of vitriol, but the quantity was only 38 grains.

As I ſuſpected that the ſelenitical ſpars contained ſome calcareous earth, not united to the acid of vitriol, I digeſted ſome of them with rectified pure acid of nitre, and afterwards well waſhed and dried them; but the loſs of weight was very trifling, except in the cauk and *erica* formed ſpar; the firſt probably containing ſome marcaſſitical particles of copper, and the laſt an ochre of iron.

Dr. LEWIS, in his tranſlation of NEWMAN'S Chemiſtry, quotes from the Philoſophical Tranſactions ſomewhat remarkable about cauk, that when wetted with antimony it gives it a ſhining ſurface like ſteel.

I repeated the experiment as follows: I powdered and mixed ℥ ſſ. of cauk with ℥ iij. of antimony, and put the mixture into a red hot crucible; and when melted, which readily happens, I ſtirred it with an iron rod, and poured it

it into an iron mortar. This matter in its fracture has appearances of RULANDUS's falſe liver of antimony.

I have alſo tried, in the ſame proportion and manner, the following ſubſtances, and found the effects ſo much alike, that they could be ſcarcely diſtinguiſhed one from the other.

Stellated ſpar, from the iſland of Sheppy.

Auvergne compact plated ſpar.

Erica formed ſpar.

Whited plated gypſum dried.

Dried whiting.

Fixed alkaly of tartar.

Hence we may conclude, that the calcareous earth of theſe ſpars and gypſum act on the antimony like fixed alkaly, forming a ſort of liver of antimony.

Of ſome mineral ſubſtances which contain the earth of allum.

Tobacco-pipe clay, of all ſubſtances I know, would be the fitteſt to make allum with, was it neceſſary; but nature has ſupplied it abundantly in other bodies, from which it is obtained with little art and expence.

Two drams of dried tobacco-pipe clay, treated with an equal quantity of fixed alkaly of tartar, as in the laſt ex-

periments, produced no tartar of vitriol; nor have I been able to obtain any from the other clays, which I ſubmitted to the ſame trials: ſuch were the porcellane clay from Cornwall, the porcellane clay from Saxony, Sturbridge clay, fuller's earth, as alſo the blue argilla from Paris, which M. BEAUME ſays is replete with the acid of vitriol[(b)], and to that he attributes its property of ſetting free the acids of ſalt petre and of ſea ſalt; but of this more fully hereafter.

The tobacco-pipe clay, having been deprived of its ſaline part after the calcination, was as white as chalk, and had loſt its tenacity[(c)]. It increaſed in weight gr. viii. and the alkaly was not only diminiſhed in weight, but was alſo combined with a portion of the clay; for, on ſaturating it with diſtilled vinegar, a gelatinous ſubſtance was ſeparated. I have often obſerved the formation of this gelatinous matter on the ſurface of the Vauxhall ſtone bottles, in which I had kept for ſome months

(*b*) M. BEAUME, in order to demonſtrate the acid of vitriol in the clay, has boiled it for a conſiderable time with fixed alkaly, and thereby obtained tartar of vitriol: had he made uſe of a pure alkaly, I would take upon me to ſay, that his experiment would have failed. The alkaly he uſed contained already tartar of vitriol, which became more manifeſt by long boiling, as a portion of the alkaly combines with the clay.

(*c*) Might not this be ſubſtituted in the room of white lead for painters uſe, the white lead having many bad qualities, and being very injurious to ſome other colours?

oil

oil of tartar, *per deliquium.* This ware is made with tobacco-pipe clay and ſand, and when well burnt is not acted on by either acids or alkalies.

This ſhews the union of clay with alkalies, and that may be the chief reaſon why it ſhould, when helped with heat, be ſo uſeful in obtaining the acids of nitre and of ſea ſalt. I have alſo obtained this gelatinous ſubſtance by mixture of tobacco-pipe clay and oil of tartar, *per deliquium*; for after ſome months, the alkaly being diſſolved with water and evaporated, had in great part a gelatinous conſiſtence. This mixture was ſtirred now and then, and had a remarkable volatile alkaline ſmell. Mr. BOYLE ſays, that clays diſtilled with ſea ſalt produces a *ſal ammoniac*, and I found it to be always true when the diſtillation is at firſt ſlowly conducted.

I have often made allum with tobacco-pipe clay calcined with oil of vitriol, but kept no notes of the quantity I obtained; and as for the *reſiduum* uſed in this experiment, it was loſt. I can, however, tell the quantity of allum which the porcellane clay from Cornwall affords. Two drams of this clay, which had been treated with its weight of fixed alkaly, and deprived of its ſaline part, then calcined four different times, with a freſh portion each time of oil of vitriol, produced ℥ ij. and gr. xxiv. of good

good cryſtallized allum[d]. The fourth calcination afforded no allum, and what remained, after being dried and waſhed, weighed gr. lv. What this matter is I have not yet tried; perhaps it may be of the quartzy kind. Hence it appears, that this clay contains half its weight of earth of allum, which, by its union with the acid of vitriol and the water that enters into its cryſtallization, produces better than four times its weight of allum, and therefore this clay treated with acid of vitriol affords more than double its weight of allum.

Of Feld ſpar.

The honourable Mr. GREVILLE, a member of the Royal Society, and remarkable for his taſte and ſkill in natural hiſtory, as well as for his judicious remarks on the nature, growth, and formation of minerals, has in his travels made ſome very intereſting obſervations on the formation of Feld ſpar; the ſpecimens which he collected on the ſpot ſhew evidently the change of clay into this ſpar, and alſo the different gradations of the change.

(d) The celebrated Mr. MARGRAF, to whom the diſcovery of making allum with *argilla* and acid of vitriol is due, could not obtain the allum in a cryſtallized ſtate without the addition of ſome fixed alkaly. His miſtake was owing to the exceſs of acid of vitriol, which the alluminous earth of the clay retained for want of ſufficient heat, and which he corrected by ſaturating it with an alkaly.

The

The Feld ſpar I made uſe of was given me by that moſt excellent chemiſt, M. ROUELLE of Paris, and I think he told me it came from Alençon.

In order to make allum of it, I melted ʒj. of it with ʒj ff. of fixed alkaly of cream of tartar, and let the mixture run *per deliquium*. This operation is as tedious and as troubleſome to perform as the *liquor ſilicum* made with flint, quartz, or cryſtal, for it is as liable to froth and boil over. No neutral ſalt was here obtained. The *deliquium* with its dregs were mixed with diſtilled vinegar, which precipitated the ſpar: this precipitate, after edulcoration and exſiccation, was calcined four different times, with freſh parcels each time of oil of vitriol, and afforded ʒvij. and gr. xvi. of good and regularly cryſtallized allum; the part which afforded no more allum, when waſhed and dried, weighed ʒj. and ℈ ij.

The Labradore ſtone [e] is alſo a Feld ſpar, though not ſo hard as the former. The like quantity of this, treated as the foregoing, gave no marks of neutral ſalt; the quantity of allum was ʒj. ʒj. and gr. xii.; and the un-

(e) This ſtone is only found on the Coaſt of Labradore, and was brought here by the direction of the Rev. Mr. LA TROBE, remarkable for his piety and zeal in propagating the goſpel among the ſavage Indians. This ſtone reflects a variety of fine ſhining colours, ſuch as blue, green, yellow, &c. I doubt not but that ſeveral other ſtones, which reflect various and changeable colours, upon trial will be found to be Feld ſpars.

/3 diſſolved part weighed ℥ v. and gr. xxxvi. This ſpar does not boil up in its fuſion with the alkaly near ſo much as the other, and therefore the loſs in the operation is trifling compared to it, and on that account it affords a greater proportion of allum.

Shirl frequently, though erroneouſly, called baſaltes.

The ſhirl I tried is of a brown colour, forms a maſs of long minute priſms cloſely adhering together, and comes from Bohemia. Its brown colour is owing to iron. This ſhirl, treated in the ſame proportion and manner as the Feld ſpar, afforded no neutral ſalt. The quantity of allum was ℥ ij. ʒ j. and gr. xxiv.; but I muſt obſerve, that the two laſt cryſtallizations contained iron, and the mother water that remained was of an oily conſiſtence, had a ſtyptic taſte, and reſembled that obtained in making green copperas. The earthy matter here remaining weighed ʒ ij. and gr. lii ; was light and of a grey colour. Hence it is evident, that ſhirl contains nearly as much earth of allum as the Corniſh porcellane clay.

Mr. ILSEMAN, an ingenious apothecary and chemiſt at Clauſthal in the Hartz, has aſſured me, that he obtained from pumice ſtone and ſhirl a *ſal catharticus amarus*. He has, I preſume, made his trials on ſhirl

which is found with lava, and is the product of a volcano. I had none of it to try, but judge it, from its appearance, to be of a different nature from that found in mines.

Allum is commonly obtained from ſlate, which for that purpoſe is calcined for a conſiderable time. I know no ſubſtance ſo replete with it as the Iriſh ſlate, *lapis Hybernicus* of the druggiſts; for this, without any calcination, affords allum.

Allum is likewiſe obtained from the earth or clay of Solfatara, and from the red ſlate found near Saarbruck; but theſe have been expoſed, no one can tell how long, to the heat of volcanos. Iron always accompanies allum, and thence the uſe of ſome alkaly, by which means the red chalk *(rubrica fabrilis)* is obtained.

Of jaſper.

Many of the jaſpers, ſo called, owe their origin to cryſtal or quartz coloured with iron; perhaps in a few inſtances to copper. Theſe, I dare ſay, treated as the former, would produce no allum; but I have only tried one of the Oberſtein jaſpers, and this had viſible marks of cryſtal. Others of the jaſpers, and theſe I call the true ones, are formed by clay and afford allum; ſuch is the

Saxon jaſper, called ribbon agate, as alſo a red one, which was given me as coming from Johngeorgenſtadt. From thoſe I obtained very good allum, but can give no account of the particulars, having loſt the paper which contained the reſult of my experiments.

Yellow pitch ſtone, and wood like calf, petrified with pitch ſtone, both from Hungary.

The reſult of my experiments on theſe ſubſtances was ſet down on the ſame paper with that of the jaſpers which was loſt; but I recollect to have only obtained a ſmall portion of allum, and for that purpoſe was obliged, before I obtained the allum well cryſtallized, to waſh away the exceſs of acid with rectified ſpirit of wine.

Tin ſpar of the Germans, *commonly called white tin ore.*

It has a ſparry appearance; but by its lamellated texture and great ſpecific gravity, which is equal to that of tin grains, is eaſily known. This is ſuppoſed by ſeveral to be rich in tin; but the Saxon mineralogiſts aſſert, that it contains none. The only experiment I made with it was to digeſt it in a powdered ſtate with acids, by which means it acquires a rich yellow colour, like turbith mi-

neral; the acid of ſalt anſwers beſt for this experiment. This is the only ſubſtance I know of which has this property.

Norway zeolite.

This ſubſtance, treated with fixed alkaly, as in the former experiments, afforded no neutral ſalt; and what remained, after the alkaly was waſhed away, treated with acids, formed the like gelatinous matter as it does in its crude ſtate.

An account of ſuch ſubſtances as have a ſparry appearance, and how to diſtinguiſh the one from the other.

Cryſtal and quartz are eaſily known by their great hardneſs, and by the copious ſparks of fire which they afford when ſtruck with ſteel.

Feld ſpar is frequently ſo hard as to ſtrike fire with ſteel, and to give copious ſparks of fire; but its laminated texture, as well as its breaking into rhombs, makes it eaſily diſtinguiſhed from cryſtal and quartz.

Phoſphoric ſpar is eaſily known by the luminous appearance it has when heated, and alſo by the ſmell it

affords when added to oil of vitriol made hot, which exactly reſembles that of acid of ſalt. It is found of various colours, as green, blue, purple, crimſon, white, and alſo yellow. When cryſtallized it forms perfect cubes; the only exception I ever met with was a ſpecimen of the green ſort, which was ſent me by Mr. SOPER, of St. Columb in Cornwall, who has diſtinguiſhed himſelf by his ſkill in natural hiſtory and mineralogy. This ſpecimen was compoſed of two quadrangular pyramids united together at their baſis.

Selenitical ſpar is found cryſtallized in a great variety of forms; is heavier than the foregoing ſubſtance; does not effervеſce or diſſolve with acids, nor is it ſo hard as the phoſphoric ſpar.

Calcareous ſpar is eaſily diſtinguiſhed from other ſubſtances by its effervеſcences and ſolution in the acids of nitre and ſea ſalt. Acid of vitriol, added to theſe ſolutions, cauſes a ſelenitical precipitation: this ſpar is neither ſo hard nor ſo heavy as the foregoing ſubſtances, and cryſtallizes in a great variety of forms.

Gypſum is lighter than any of the foregoing ſubſtances, and is ſo ſoft as to be eaſily ſcratched with one's nail; it does not effervеſce nor diſſolve with acids, and is the only ſubſtance that forms plaſter when burnt; it cryſtallizes in a great variety of forms.

Mica or Glimmer. This is lighter than any of the ſparry ſubſtances; is compoſed of very thin flexible flakes, more or leſs large. Many of this kind are found in form of ſmall coloured ſcales of various colours, and very much reſemble the bronzes.

Tin ſpar, or white tin ore, ſee its deſcription heretofore.

White lead ore is found of a great variety of forms; is very heavy, effervesces with the acids of nitre and ſea ſalt, and totally diſſolves in them, with the help of heat. With the acid of ſalt it forms cryſtals much reſembling a ſilver coloured glimmer, juſt as common lead would have done, and with the acid of nitre it forms regular cryſtals.

The ſpathoſe iron ores, when powdered and put on a red-hot iron or ſtone, inſtantly become black, and look like a black, ſhining, micaceous iron ore.

Zeolite is lighter than the calcareous ſpars: ſee its properties already deſcribed.

Of a ſet of ſpars whoſe properties were not hitherto known, and experiments made on one of them.

This ſpar cryſtallizes in form of flat, and alſo of ſolid, rhomboidal cryſtals, and is found of a great variety of colours, ſuch as white and ſemi-tranſparent, of a pearl colour,

colour, reddiſh, and of different ſhades of brown and yellow, ſome of them being like gold, braſs, and copper. It has always a peculiar gloſs or brightneſs. May not the green and yellow glimmers from Johngeorgenſtadt be of this kind?

The ſpecimen I made uſe of for the following experiments is in Dr. HUNTER'S collection, and ſeemed to be well adapted for this purpoſe, it being perfectly free from any matrix or heterogeneous matter. The whole ſubſtance of it is a cruſt compoſed of rhomboids, which grow out of one another, and form a variety of cavities: its colour is white and ſemi-pellucid, and on one ſide there was a ſlight marcaſitical coating. This ſubſtance comes from Joachimſthal, and is harder than any of thoſe mentioned before, except the cryſtal, quartz, and feld ſpar.

EXPERIMENT I.

Three drams of this ſpar, with an equal quantity of fixed alkaly of cream of tartar, melt with a moderate degree of fire; but the mixture ſoon becomes thick and cakes. It was after an hour's calcination ſuffered to cool, then was powdered and deprived of its ſaline part with boiling diſtilled water. This ſaline part, treated as in the other

other experiments with diſtilled vinegar and rectified ſpirit of wine, gave no marks of neutral ſalt. The undiſſolved part after this operation dried, weighed ʒ ij. and gr. l.: this was mixed with oil of vitriol, which cauſed a ſtrong effervescence, and then calcined, to be deprived of its exceſs of acid. It was now digeſted, at three different times, with diſtilled water, which diſſolved a portion of it; what remained undiſſolved after this operation, being dried, weighed ʒ j. and gr. lii. and was a ſelenite, as will appear by further experiments. The three portions of water, with which this matter was digeſted, mixed together, then evaporated and cryſtallized, produced ʒ ij. and gr. xlv. of a white ſalt, moſtly conſiſting of rhomboidal priſms, ſome lying flat and ſome erected ſideways. This I judged, by its ſtyptic taſte, to contain iron, and by its white colour to contain a *ſal catharticus amarus*, or at leaſt a new earth, which with the acid of vitriol forms a ſoluble ſalt.

EXPERIMENT II.

This ſpar, in its crude ſtate, effervesces ſtrongly with oil of vitriol diluted with water. Three drams of the ſpar treated in this manner, and afterwards deprived by calcination of its exceſs of acid, and then of its ſaline part by diſtilled

diſtilled water, left a ſelenite, which when dried weighed ℥ ij. and gr. xx. This proportion is greater than in the former experiment, which I attribute to the acid of vitriol having been diluted with water, and its combining on that account in a greater quantity with the calcareous earth of the ſpar. The ſaline part by evaporation and cryſtallization produced ʒij. and gr. xxviii. of ſalt, compoſed of ſmall long cryſtals, like Epſom ſalts; its taſte was bitter and ſtyptic.

This ſpar, in its natural ſtate, effervesces ſtrongly with rectified acid of ſalt, produces heat, and totally diſſolves in it; the ſolution is of a fine yellow colour.

Rectified acid of nitre diſſolves alſo this ſpar with effervesence and heat; but the ſolution is colourleſs.

I judged from the foregoing experiments that this ſpar was compoſed of calcareous earth, ſome iron, and a portion of ſome other earth, which, with acid of vitriol, forms a ſoluble ſalt.

EXPERIMENT III.

In order to determine the quantity of calcareous earth this ſpar contained, I diſſolved three drams of it in rectified acid of ſalt, and by the addition of a ſufficient quantity of a ſolution of tartar of vitriol in water, I obtained a ſelenite,

a ſelenite, which, when waſhed and dried with a ſtrong heat, weighed ℥ iij. and gr. xxxvi.

Three drams of dried whiting, diſſolved in the ſame acid, and treated in the like manner, afforded ℥ iv. and gr. xviii. of ſelenite. The whiting was all diſſolved, except three grains. The calcareous earth contained in the ſpar muſt, by its formation into ſelenite, be increaſed in weight in the ſame proportion as the whiting; and hence it is evident, that three drams of this ſpar contains ℥ j ſſ. and gr. xix, of calcareous earth. The remaining part muſt be the earth above-mentioned, with ſome iron.

EXPERIMENT IV.

In order to judge of the quantity of iron that this ſpar contained (not having any more of it left) I took the ſalt of the firſt experiment, and diſſolved it with water, to which I added ſome acid of ſalt. I then precipitated the iron in form of Pruſſian blue with the common alkaline *lixivium* uſed for that purpoſe, and thereby obtained ſixteen grains of a fine deep-coloured blue. Acid of ſalt was added in the uſual manner, to heighten the colour, by which means nothing but iron was precipitated.

Two drams of common green copperas, treated in the ſame manner, produced ℥ j. leſs ~~than~~ two grains of the

ſame ſort of blue, though not ſo good. Hence we may conclude, that the ſalt of the firſt experiment, which was produced by three drams of ſpar with acid of vitriol, contained thirteen grains of vitriol of iron.

Pruſſian blue, made without help of allum, contains nearly half its weight of iron. Hence, from the foregoing experiments, three drams of this ſpar contain gr. viii. of iron, ʒj. and gr. xlix. of calcareous earth, and ʒj. gr. iij. of the earth of *ſal catharticus amarus*, or perhaps ſome other earth, which forms, with acid of vitriol, a ſoluble ſalt.

The ſpathoſe iron ore being frequently found cryſtallized like the foregoing new ſpar, and having alſo a gloſs on it, I was willing to try whether it had any affinity with it; but by the following experiments it appears to be of a different nature, not containing any calcareous earth.

The ſpathoſe iron ore diſſolves almoſt totally in acid of ſalt, and the ſolution is of a deep yellow colour. A ſolution of tartar of vitriol in water added to it cauſes no precipitation; and hence it is evident, that it contains no calcareous earth. Acid of vitriol treated with the ſpathoſe iron diſſolves the whole of it, excepting a few dregs; another proof of its containing no calcareous earth. The acid of nitre diſſolves alſo this ſpar, and the ſolution is colourleſs.

IV. *Account of a Petrefaction found on the Coaſt of* Eaſt Lothian. *By* Edward King, *Eſq. F. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

John-ſtreet,
November 14, 1778.

Read Nov. 26, 1778.

IN conſequence of the honour you did me, to put into my hands a very curious ſpecimen of a recent petrefaction, permit me now to offer to your conſideration a few thoughts concerning this production, which have occurred to me on comparing it with others of a ſimilar kind, and which may at leaſt ſerve as hints for further inveſtigation.

We ſhould not venture, it is true, without great caution, to ſpeculate on theſe matters, as haſty and ſpecious concluſions may eaſily be drawn by any one who indulges too readily a quick and lively imagination, which will ever be too ready to miſlead, rather than to procure ſolid information. But though I

am well aware of this danger, yet I venture to lay before you theſe few obſervations; and if you judge them at all worthy of attention, I would wiſh to communicate them, through your hands, to the Royal Society, for they are not made merely in conſequence of a ſlight and haſty ſurvey of this one ſpecimen, but are in truth concluſions that I have been led to form incidentally in the courſe of a very long inquiry, which I have been for ſome years purſuing on another occaſion; the reſult whereof I ſhall perhaps, if I live, hereafter communicate to the Society in a more full and explicit manner than the compaſs of a paper of this kind will permit.

The account of this ſpecimen, with which you favoured me, is as follows. In the year 1745, the Fox man of war was unfortunately ſtranded on the coaſt of Eaſt Lothian in Scotland, and there went to pieces; and the wreck remained about three and thirty years under water; but this laſt year a violent ſtorm from the North-eaſt laid a part of it bare, and ſeveral maſſes, conſiſting of iron, ropes, and balls, were found on the ſands near the place, covered over with a very hard ochry ſubſtance, of the colour of iron, which adhered thereto ſo ſtrongly, that it required great force to detach it from the fragments of the wreck. And, upon examination, this ſub-

ſtance

ſtance appeared to be ſand, concreted and hardened into a kind of ſtone.

The ſpecimen now laid before the Society had been taken out of the ſea, from the ſame ſpot, ſome time before, and is a conſolidated maſs that had undergone the ſame change. It contains a piece of rope that was adjoining to ſome iron ring, and probably had been tied thereto. The ſubſtance of the rope is very little altered; but the ſand is ſo concreted round it, as to be as hard as a bit of rock, and retains very perfectly impreſſions of parts of the ring, juſt in the ſame manner as impreſſions of extraneous foſſil bodies are often found in various kinds of ſtrata.

Now, conſidering theſe circumſtances, we may fairly conclude, in the firſt place, that there is, on the coaſts of this iſland, a continual progreſſive induration of maſſes of ſand and other matter at the bottom of the ocean, ſomewhat in the ſame manner as there is at the bottom of the Adriatic ſea, according to the account given by Dr. DONATI[a].

And, in the next place (which is what more particularly deſerves our attention on this occaſion), it ſhould ſeem, that iron, and the ſolutions of iron, contribute very much to haſten and promote the progreſs of the concretion and induration of ſtone, whenever they meet and are

(a) See the Phil. Tranſ. vol. XLIX. p. 588.

united

united with thoſe cementing cryſtalline particles, which there is reaſon to believe are the more immediate cauſe of the conſolidation of all ſtones and marbles whatſoever, and which do very much abound in ſea water.

It would exceed the limits of this paper, were I to attempt here to mention only a ſmall part of the various facts that have come to my knowledge; and which have convinced me, and I truſt, when offered fairly to the conſideration of others, will make it appear fully to all that are attentive to theſe kind of reſearches, that there is, to this very day, a formation of ſtone, and even of marble itſelf, in certain places, in a much more perfect manner, than has been generally conceived; and far beyond what has been ſuppoſed, even by thoſe who have been ready too haſtily to account for ſuch a proceſs merely in conſequence of obſervations made on ſtalactitical and ſuch like ordinary concretions.

I ſhall not therefore preſume to trouble the Society, at preſent, with any detail of the inquiries I have made relating to that ſubject, though in reality they have been the foundation of the obſervations made in this paper; but ſhall confine my remarks merely to this one curious circumſtance; that wherever there is any induration and petrefaction of matter, from any cauſes whatever, it is greatly haſtened in its progreſs, and the conſolidation is rendered much more compleat and firm by being

being near any maſs of iron, and ſtill more ſo by the admixture of any ſolution of that metal.

This appears, in ſome degree, from the preſent ſpecimen; where, near adjoining to the ring, and in the portion of the fragment that has the largeſt impreſſion thereof, the concreted ſand-ſtone is of a firmer texture, and there is a larger cohering maſs formed about that part of the rope, than about thoſe parts that are further removed from the ring.

It appears alſo from a circumſtance that was particularly taken notice of when the wreck was diſcovered by the ſtorm this year (and which is mentioned, SIR, in the letter you favoured me with a ſight of); for the maſſes that were obſerved to have theſe concretions adhering to them, were not maſſes of timber, or other large fragments of the wreck, which one would think, on a ſlight conſideration of the matter, were moſt likely to cauſe obſtructions at the bottom of the ocean, and to form little banks of ſand, that might afterwards be concreted; but they were maſſes of looſe iron and ropes, and even of cannon balls, which were thus conſolidated.

The ſame concluſion alſo may be drawn, with ſtill more appearance of its being well founded, from a very remarkable piece of antiquity, which was diſcovered about three years ago on the coaſt of Kent. Some fiſhermen, ſweeping for anchors in the Gull ſtream (a part of

the ſea near the Downs), drew up a very curious old ſwivel gun, near eight feet in length. The barrel of the gun, which was about five feet long, was of braſs; but the handle (whereby it was to be turned or traverſed) which was about three feet in length, and alſo the ſwivel and pivot on which it turned, were of iron, and all round theſe latter, and eſpecially about the ſwivel and pivot, were formed exceeding hard incruſtations of ſand, converted into a kind of ſtone, of an exceeding ſtrong texture and firmneſs; whereas round the barrel of the gun, except where it was near adjoining to the iron, there were no ſuch incruſtations at all, the greater part of it being clean, and in good condition, juſt as if it had ſtill continued in uſe[b].

The incruſtation round the iron part of this gun was alſo the more deſerving of attention, becauſe it incloſed within it, and alſo held faſtly adhering to it on the outſide, a number of ſhells and corallines, juſt in the ſame manner as they are often found in a foſſil ſtate. There were plainly to be diſtinguiſhed, on the outſide of this maſs of incruſtation, pectens, cockles, limpets, muſcles, *vermiculi marini* and *balini*; and beſides theſe, one *buccinum*

(*b*) As there were ſeveral remarkable particularities in this gun, tending to explain ſome curious facts in hiſtory, I took the liberty to give a full account of it, with a view to illuſtrate them, in a memoir laid before the Antiquarian Society laſt year.

num[c] and one oyſter; and they were all ſo thoroughly and ſtrongly fixed thereto, and themſelves alſo converted into ſuch an hard ſubſtance, that it required as much force to ſeparate or break them, as to break a fragment off any hard rock; and in colour and appearance, they much reſembled ſome of the maſſes of foſſil bodies found near Chippenham in Wiltſhire.

This remarkable incruſtation, therefore, thus confined to the parts of the gun adjoining to the iron, and appearing no where elſe upon it, plainly indicates, that the iron was, by ſome means or other, the more immediate cauſe thereof: and yet it is to be obſerved, that in this inſtance the iron was very little diſſolved; for although it is manifeſt, from ſome circumſtances in its hiſtory, that the gun muſt have remained in the ſea above two hundred years, and probably a great deal longer, yet the greater part of the handle and of the ſwivel remained entire, and even the point of the pivot was undiſſolved, and very viſible.

Another curious appearance alſo, of a ſimilar nature, will tend further to confirm the obſervations here made. This is found in a ſpecimen (now in my poſſeſſion) of a moſt remarkable incruſtation, that was formed in the ſpace of three years only, within a ſquare wooden pipe, in a Coal Mine in Somerſetſhire. I gave a very particu-

(c) See the figure marked G 41.

lar account of this production in a paper laid before the Society in the year 1773[d], and therefore ſhall ſay nothing of it here but what immediately relates to the preſent purpoſe. The pipe was forty-two feet in length, and the hollow part of it was ſeven inches and an half wide, by four inches and an half; and this whole cavity was ſo filled with the incruſted matter (which was hard enough to take an exceeding fine poliſh, like the moſt beautiful marble) that at laſt there was left a water way, which, nearly uniformly throughout the whole length of the pipe, was only about three inches and an half by one inch; and thus there was formed, within the firſt wooden pipe, a ſecond pipe of this incruſtration, the thickneſs of the ſides of which was about one inch and an half. On cutting a tranſverſe ſection of this pipe there appeared a number of uniform lines, forming almoſt regular ſimilar parallelograms, one within another, like the coats of an onion, and plainly denoting the gradual and regular progreſs of the formation of the whole incruſtation. But the circumſtance moſt remarkable, and that is more immediately applicable to the preſent purpoſe, is, that where there was, by accident, the point of a nail projecting through the ſide of the wooden pipe, it ſo accelerated the progreſs of the incruſtation, that, adjoining thereto, the ſimilar ſides of the firſt and outward

(d) Phil. Tranſ. vol. LXIII. p. 241.

parallelograms next the wooden pipe (inſtead of continuing as ſtraight lines) formed in that place very bold ſemi-circular curves, or protuberances, one beyond another, projecting from the wooden pipe inwards; and this curvilinear projection was uniformly continued throughout all the ſimilar parallelograms quite to the inward cavity of the pipe formed by the incruſtation, and there at laſt occaſioned a projecting gibboſity, of a conſiderable extent every way, from the point of the nail[e].

Having mentioned theſe remarkable facts to my very learned and ingenious friend Dr. FOTHERGILL, I had the pleaſure to find they ſtruck him much in the ſame light in which they had appeared to me; and that he, moreover, formed the ſame concluſion concerning the ſpecimen now laid before the Society that you had alſo formed and mentioned in the note you favoured me with when you ſent the ſpecimen to my houſe; namely, that the concretion was effected by the ſolution of the adjoining iron ring.

Dr. FOTHERGILL alſo (who had communicated ſome very original conjectures upon this ſubject to the Society many years[f] ago) informed me of ſome further

(e) There is another fragment of the incruſtation formed within this pipe, with a tranſverſe ſection thereof poliſhed, in the Muſeum of the Royal Society; but that with the point of the nail, which is here alluded to, is ſtill in my poſſeſſion.

(f) In a paper on the Origin of Amber, read in the year 1743.

curious facts, which he permits me now to mention in this paper.

On paſſing through the ſtreets of London in his walks, before the ſign-irons were taken down, he perceived, that on the broad ſtone pavements, whenever he came juſt under any ſign-irons, his cane gave a different ſound, and occaſioned a different kind of reſiſtance to the hand, from what it did elſewhere; and attending more particularly to this circumſtance, he found, that every where, under the drip of thoſe irons, the ſtones had acquired a greater degree of ſolidity, and a wonderful hardneſs, ſo as to reſiſt any ordinary tool, and gave, when ſtruck upon, a metallic ſound: and this fact, by repeated obſervations, he was at length moſt thoroughly convinced of.

Taking the hint, therefore, from hence, he thought fit to make ſeveral experiments; and, amongſt the reſt, placed two pieces of Portland ſtone in the ſame aſpect and ſituation in every reſpect, but waſhed the one frequently with water impregnated with ruſty iron, and left the other untouched: and in a very few years he found the former had acquired a very ſenſible degree of that hardneſs before deſcribed, and on being ſtruck gave the metallic ſound; whilſt the other remained in its original ſtate, and ſubject to the decays occaſioned by the changes

of

of the weather, which we find in many inſtances make a moſt rapid progreſs.

He alſo mentioned to me a very curious circumſtance of his having found on the ſea-coaſt near Scarborough, many years ago, part of an horſe-ſhoe incruſted with ſea ſand, which was ſo concreted as to have acquired the hardneſs of common grit ſtone, and retained the colour of the ſand, with very little tincture of the iron ochre: and by the part which was left free from the incruſtation it appeared moſt probable, that the horſe-ſhoe had not been buried there many years, but had very recently acquired this incruſtation upon that part only that was moſt expoſed to the waſhing of the ſea water.

Now, all theſe facts put together ſcarce leave any doubt but that iron, and ſolutions of iron, do grealy promote and haſten the progreſs of all kinds of petrifaction; and therefore, to purſue Lord BACON's rule of induction, deducing truth from a variety of facts and experiments, all manifeſtly and uniformly leading us to the ſame concluſion, we may fairly infer, that whereas iron is of ſuch manifeſt uſe in the progreſs of vegetation, that plants are indebted for their green colour[g], and for

(g) There are ſome exceeding curious experiments and obſervations of Mr. DELAVAL's, tending to illuſtrate this fact, in the Phil. Tranſ. vol. LV. p. 36, 37.

many of their valuable qualities, to its being intimately mixed in their ſubſtance; and as it is moreover obviouſly uſeful, ſome way or other, in the animal ſyſtem, and may be extracted by the magnet from the aſhes of animal ſubſtances; ſo it is no leſs uſeful in the conſolidation of ſtones and marble in the foſſil world.

Mr. PRYCE, in a very uſeful and curious treatiſe of Mineralogy[(b)], has moreover lately ſhewn it to be equally uſeful in the mineral world, by forming a proper *nidus* for the aſſemblage of the moſt valuable metals, and attracting and uniting them thereby. This metal, therefore, ſeems to be almoſt univerſally one of the greateſt bands that unites the ſeveral parts of matter, and one of the moſt uſeful and important of ſubſtances in the world.

It is not for us to preſume to comprehend any thing about the original formation of bodies. Such diſquiſitions are far out of the reach of our faculties; nor do I at all pretend to enter into them: but we are permitted to behold and conſider the works of the Almighty, and may become wiſer and reap profit from the contemplation of them, and may perceive in what manner many new combinations of matter are continually effected.

And as we manifeſtly perceive plants to grow daily for the neceſſary ſupplies of life, without knowing how

(*b*) Mineralogia Cornubienſis, p. 6. 11. 67.

they

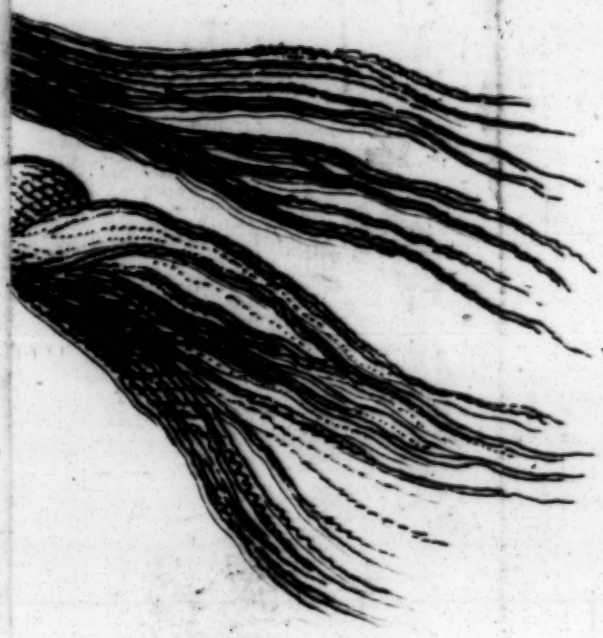


Fig: II.

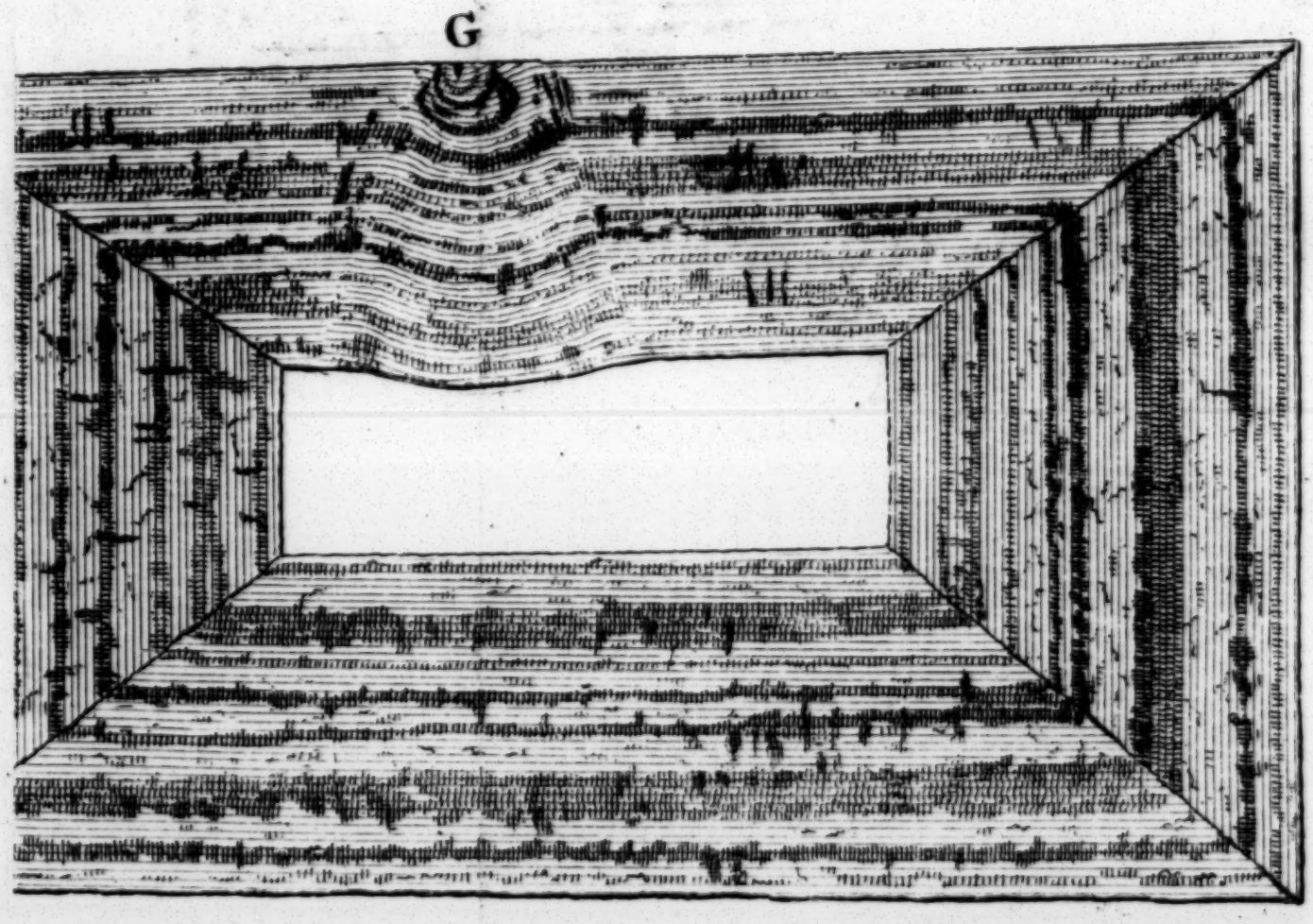


Fig: I.

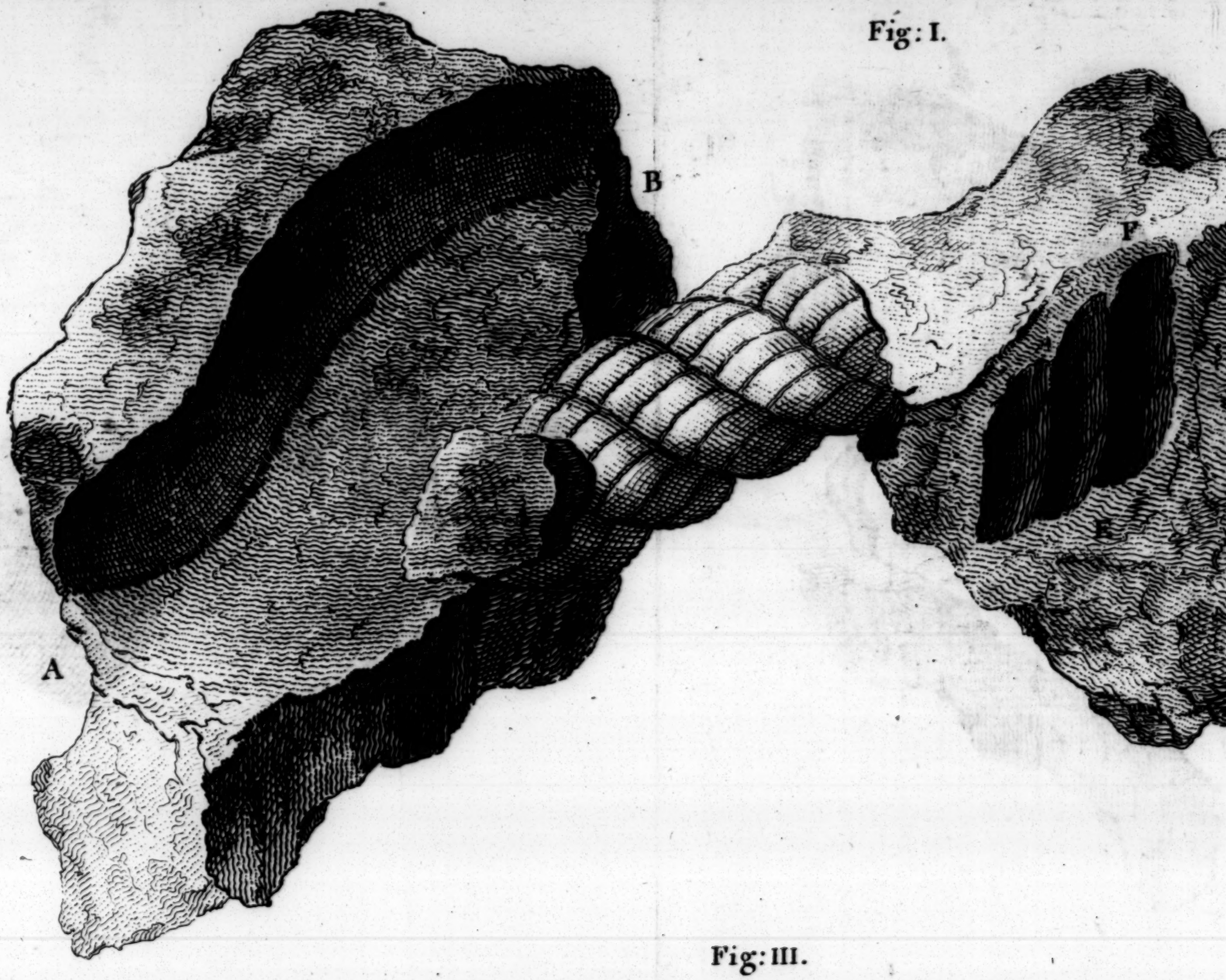
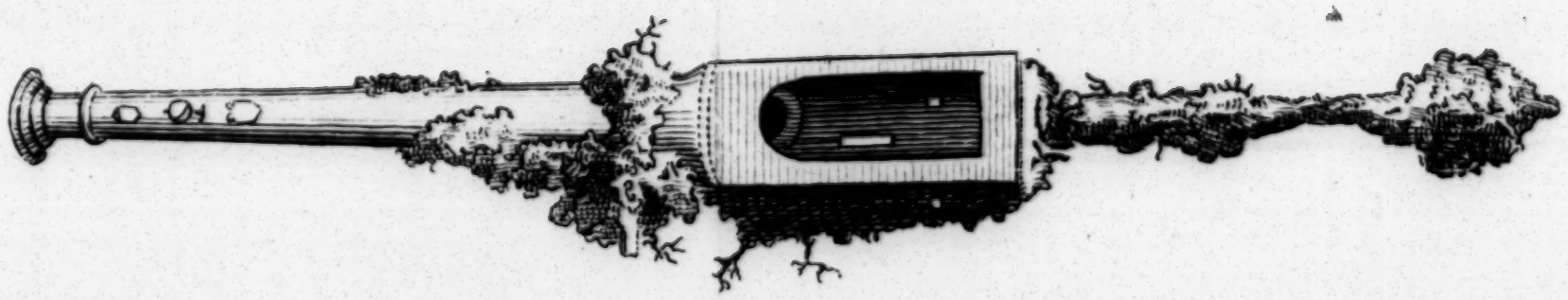


Fig: III.



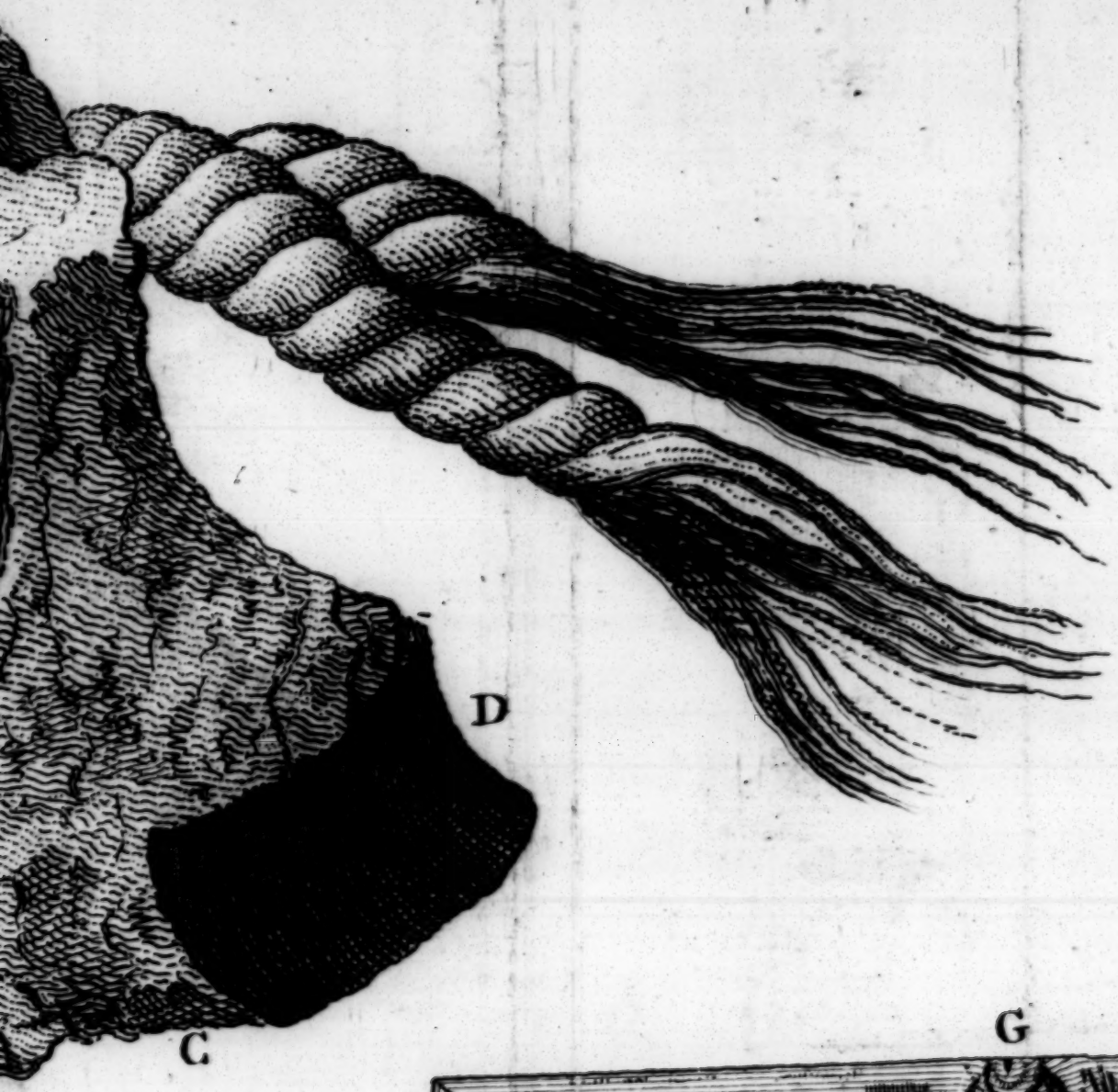
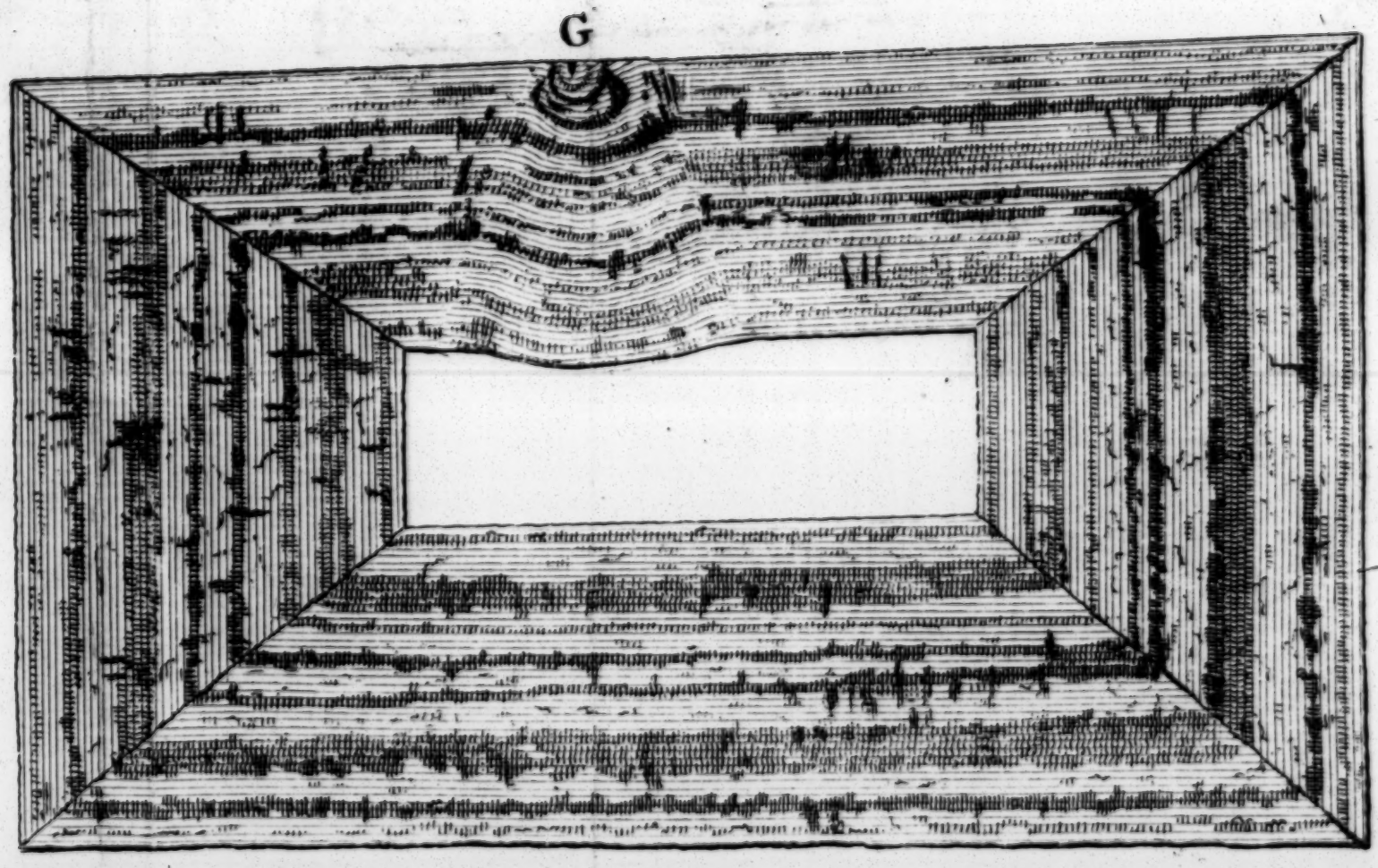


Fig: II.



they grow; but yet are convinced they are nouriſhed by means of the ſalts and particles of matter conveyed to them by water and vapour, and that iron is a neceſſary ingredient in their compoſition; ſo I am perſuaded we ſhall, at length, perceive (notwithſtanding the general opinion to the contrary) that even ſtones and marbles are ſtill continually forming in the earth for the ſervices of human life, and to ſupply the continual waſte and decay that there is of thoſe ſubſtances; and that the conſolidation of them is effected by means of water and vapour likewiſe (of which I am able to produce very many and convincing proofs); and that iron, which is what alone I wiſh now to make apparent, is unqueſtionably a principal means of effecting the induration of many of thoſe bodies.

But I would not willingly trouble the Society with this paper as a matter of mere ſpeculation. I would wiſh that ſome hint, which might be more immediately uſeful, ſhould, if poſſible, be derived therefrom; and ſuch has been ſuggeſted to me by Dr. FOTHERGILL.

If iron and the ſolutions of iron do thus contribute to the induration of bodies, ſuch ſolutions muſt probably have that tendency in every ſtage of thoſe bodies' exiſtence; and therefore it ſeems likely, that the fine ornamental carvings in Portland, or other ſtone, might be

much

much hardened, and preſerved for a much longer time than has been uſual, from the injuries of the weather, by being waſhed and bruſhed over by water, in which is infuſed a ſolution of iron. And perhaps even the ſofter kinds of ſtones might have been preſerved by this means; and the venerable remains of that fine pile of building HENRY the VIIth's chapel, might have been ſaved from the deſtruction with which we now ſee it ready to be overwhelmed.

It is very probable, moreover, that common ſea ſand, with a very ſmall admixture of a ſolution of iron, may at length, without any great expence, be converted into a moſt uſeful ſpecies of ſtone, and be applied to the purpoſe of covering the fronts of houſes even more durably, and in as beautiful a manner as ſome of the late invented ſtuccos; and even thoſe ſtuccos may be improved by means of the ſame mixture.

It muſt be left to future experiments to aſcertain what proportions of the ſolutions of iron are beſt to be made uſe of; and in what manner the ſolution may beſt be obtained for this purpoſe; whether by a vitriolic acid; or merely by laying ruſty iron for ſome time in water[(i)]. But one caution ſhould be obſerved; namely, that if too

(i) It is not perhaps poſſible to contrive a leſs ſaturated tincture of iron than the ruſt diſſolved in water.

great

great a proportion of vitriolic acid be left in the mixture, it may do more miſchief than the iron can do good. With this caution it were perhaps much to be wiſhed, that experiments[(h)] ſhould be made, and attended to, for a long courſe of years; for without ſuch continued and repeated trials, it will be impoſſible to determine in what manner the application may be made with moſt advantage.

I have only therefore to add, that what has been ſaid may perhaps receive ſome further confirmation, from conſidering that the chief conſtituent parts of all cements for ſtone are always lime and iron[(i)]; and that any experiments made in conſequence of theſe obſervations will be likely to ſucceed more rapidly in warm climates than in colder ones.

Whatever there is of inaccuracy in this ſhort paper will I am perſuaded meet with indulgence both from you and from the Society, whilſt whatever truth is brought to light thereby will be received and adopted. I therefore venture to deliver it into your hands without heſitation. I am, &c.

(h) The ages of modern buildings are eaſily known, and there are none but what have iron rails or bars about them expoſed to the weather. If the rain drops from them on Portland or any gritty ſtone, it becomes harder. The length of time ſuch ſtones have been ſubjected to this proceſs may eaſily be known, and the effects produced in them may be verified by repeated compariſons of different fragments.

(i) See CROUSTEDST's Mineralogy, p. 45.

I have added a few rough ſketches to explain what has been ſaid in this letter.

FIG I. Repreſents the maſs of concreted ſand and rope from the coaſt of Lothian.

AB ſhews where there is one impreſſion of a part of the iron ring.

CD ſhews were there is alſo another impreſſion of the ring; and

EF ſhews where there alſo remain ſome impreſſions of bits of untwiſted rope, ſo that this whole maſs ſeems to have been formed within the circumference of the ring.

FIG. II. Repreſents a tranſverſe ſection of the petrefaction within the wooden pipe, from the coal mine in Somerſetſhire; and

G ſhews the place of the point of the nail, and the regular curvilinear projection of the petrefaction around it, regularly throughout the whole progreſs.

FIG. III. Is a ſketch of the gun, ſhewing the parts to which the incruſtation adhered.

V. *Account of Dr.* Knight's *Method of making artificial Loadſtones. By Mr.* Benjamin Wilſon, *F. R. S.*

TO JOSEPH BANKS, ESQ. P. R. S.

SIR,

Read Dec. 17, 1778.

THE method of making artificial loadſtones, as it was diſcovered and practiced by the late Dr. GOWIN KNIGHT, being unknown to the public; and I myſelf having been frequently preſent when the doctor was employed in the moſt material ſteps of that curious proceſs, I thought a communication thereof would be agreeable to you and the philoſophic world.

The method was this: having provided himſelf with a large quantity of clean filings of iron, he put them into a large tub that was more than one-third filled with clean water: he then, with great labour, worked the tub to and fro for many hours together, that the friction between the grains of iron by this treatment might break off ſuch

ſmaller parts as would remain ſuſpended in the water for a time. The obtaining of thoſe very ſmall particles in ſufficient quantity ſeemed to him to be one of the principal *deſiderata* in the experiment.

The water being by this treatment rendered very muddy, he poured the ſame into a clean earthen veſſel, leaving the filings behind; and when the water had ſtood long enough to become clear, he poured it out carefully, without diſturbing ſuch of the iron ſediment as ſtill remained, which now appeared reduced almoſt to impalpable powder. This powder was afterwards removed into another veſſel, in order to dry it; but as he had not obtained a proper quantity thereof, by this one ſtep he was obliged to repeat the proceſs many times.

Having at laſt procured enough of this very fine powder, the next thing to be done was to make a paſte of it, and that with ſome vehicle which would contain a conſiderable quantity of the phlogiſtic principle; for this purpoſe he had recourſe to linſeed oil in preference to all other fluids.

With theſe two ingredients only he made a ſtiff paſte, and took particular care to knead it well before he moulded it into convenient ſhapes. Sometimes, whilſt the paſte continued in its ſoft ſtate, he would put the impreſſion

impreſſion of a ſeal upon the ſeveral pieces; one of which is in the Britiſh Muſeum.

This paſte was then put upon wood, and ſometimes on tiles, in order to bake or dry it before a moderate fire, at a foot diſtance or thereabouts.

The doctor found, that a moderate fire was moſt proper, becauſe a greater degree of heat made the compoſition frequently crack in many places.

The time required for the baking or drying of this paſte was generally five or ſix hours before it attained a ſufficient degree of hardneſs. When that was done, and the ſeveral baked pieces were become cold, he gave them their magnetic virtue in any direction he pleaſed, by placing them between the extreme ends of his large magazine of artificial magnets for a few ſeconds or more, as he ſaw occaſion.

By this method the virtue they acquired was ſuch, that when any one of thoſe pieces was held between two of his beſt ten guinea bars, with its poles purpoſely inverted, it immediately of itſelf turned about to recover its natural direction, which the force of thoſe very powerful bars was not ſufficient to counteract.

I am, &c.

VI. *Account of an extraordinary Dropſical Caſe. By Mr.* John Latham, *in a Letter to Mr.* Warner, *F. R. S.*

TO MR. WARNER.

SIR,

Dartford, October 28, 1778.

Read Dec. 17, 1778.

WHEN I laſt had the pleaſure of ſeeing you, it was your opinion, that the Royal Society would receive ſome ſatisfaction in my giving ſome account of the caſe of Miſs A. M. who died lately of a dropſy under my care.

This patient was of a florid, lively conſtitution, but from a child was ſubject to a violent eruption, which came generally on a ſudden, covering the whole neck, breaſt, and often great part of the face; and after remaining a week or two abated in violence, and went off by degrees. The intervals were uncertain, but for the moſt part in ſpring and in autumn ſhe was more apt to have it, though frequently three or four times in the year. Various methods were tried to eradicate this com-

plaint without effect; nor did the appearance of the menſes, as we had ſome reaſon to hope, in the leaſt turn out in her favour. It will be needleſs to relate here the various medicines which had been given her with little or no ſucceſs, except that the moſt relief ſhe found was from the uſe of ſalt water, which was thought to make the intervals the longer in two or three inſtances, as well as the appearance of the eruption milder. Things continued thus till the autumn 1773, when the menſes became obſtructed, continuing ſo for ſome months, but appeared once more very plentifully; after which they never returned, neither did the eruption, except in the moſt trifling manner. About Chriſtmas 1773 ſhe complained of a weight in the abdomen, and fulneſs of the ſtomach; which ſymptoms were relieved by ſome gentle opening medicines. She then went on a viſit to ſome friends at a diſtance, after which I ſaw her no more for two months. I learned, that during that time the complaints had returned more violent, for which ſhe conſulted a phyſician on the ſpot, but without the relief ſhe found at firſt; for the abdomen began to increaſe in ſize every day, and became painful, the urine high-coloured, and in ſmall quantity, with thirſt, and every other ſymptom of an approaching dropſy.

In

In a narrative of this kind it may be expected, that a detail of the medicines ſhe took during her illneſs might be noted; but as I chiefly acted in my ſurgical capacity, and as ſhe was after this time, till the firſt operation, for the moſt part in London, under the care of phyſicians of the firſt eminence, it is out of my power to give ſuch an account; ſuffice it then to ſay, that ſhe was obliged to ſubmit to the operation of the *paracenteſis* the 27th of June, 1774. The quantity I then took off was only twelve pints, ſomewhat fœtid, but not very dark coloured, nor was it ever after the leaſt offenſive. The operation was repeated in ſix weeks, when twenty-nine pints were taken off; after that time once in four weeks to the end of the year. During the whole of the year 1775 I tapped her once in a fortnight more or leſs; and in the year 1776 ſhe for the moſt part underwent the operation every eight or nine days, the intervals gradually ſhortening, till by the end of the year ſhe could go no longer than a week between, which continued to the day of her death, which happened May 14, 1778, being then not quite twenty-three years of age. About a week before that time, ſhe was troubled with inceſſant vomitings, which nothing would relieve; but was better a few hours before her death, and went off pretty eaſy.

I have good reaſon to ſuppoſe the complaint originated from a diſeaſe of the left ovary, for after the firſt tapping I felt a ſubſtance of the ſize of a cricket ball; and, as the operations went on, this became more and more manifeſt, increaſing ſo much as at laſt to occupy the whole ſpace of the *abdomen* forward, being of a very irregular form, and I am clear of many pounds weight, for ſhe appeared, even after the water had been drawn off, as large as a woman in the laſt month of pregnancy. It would have added greatly to my ſatisfaction to have been able to clear up this point in every particular, by opening her after death; but I had the extreme mortification of being denied this neceſſary circumſtance, notwithſtanding my moſt earneſt ſolicitations.

I muſt, therefore, content myſelf with giving this bare recital of facts as above, which will ſerve to record to futurity, a caſe which I believe has not its equal in regard to the number of operations. What is remarkable here is, that this young lady had a good appetite for the moſt part, and was very chearful; and, except a day before and after each operation, uſed to viſit her friends at ſeveral miles diſtance as ſhe would have done in health, and till within the laſt two or three months could walk a mile or two with tolerable eaſe.

As to the quantity of water taken off, I find it to amount, upon the neareſt calculation, to twenty-four pints at each operation; for though the firſt time produced only twelve pints, and in ſeveral of the latter operations the quantity fell ſhort of twenty-four pints, yet I may venture to ſtate it at leaſt at twenty-four pints or three gallons on an average, as in many of the operations I took off from twenty-eight to thirty pints. The number of times I tapped her was in all 155, which brings out in the whole 3720 pints, being 465 gallons, not far ſhort of ſeven hogſheads and an half. As to the authenticity of the whole, your connections with the family, and frequent opportunities of ſeeing this young lady during her illneſs, will put it beyond a doubt. I have therefore no more to add, than my wiſh that the caſe may prove acceptable to the Society.

I am, &c.

VII. *Problems concerning Interpolations.* *By* Edward Waring, *M. D. F. R. S. and of the Inſtitute of* Bononia, Lucaſian *Profeſſor of Mathematics in the Univerſity of* Cambridge.

Read Jan. 9, 1779.

MR. BRIGGS was the firſt perſon, I believe, that invented a method of differences for interpolating logarithms at ſmall intervals from each other: his principles were followed by REGINALD and MOVTON in France. Sir ISAAC NEWTON, from the ſame principles, diſcovered a general and elegant ſolution of the abovementioned problem: perhaps a ſtill more elegant one on ſome accounts has been ſince diſcovered by Meſſ. NICHOLE and STIRLING. In the following theorems the ſame problem is reſolved and rendered ſomewhat more general, without having any recourſe to finding the ſucceſſive differences.

THEOREM I.

Aſſume an equation $a+bx+cx^2+dx^3 \ldots\ldots x^{n-1}=y$, in which the co-efficients a, b, c, d, e, &c. are invariable;

let α, β, γ, δ, ε, &c. denote n values of the unknown quantity x, whoſe correſpondent values of y let be repreſented by s^{α}, s^{β}, s^{γ}, s^{δ}, s^{ε}, &c. Then will the equation $a+bx+cx^2+dx^3+ex^4 \ldots x^{n-1}=y=$

$$\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}\times s^{\alpha}+\frac{\overline{x-\alpha}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\beta-\alpha}\times\overline{\beta-\gamma}\times\overline{\beta-\delta}\times\overline{\beta-\varepsilon}\times\&c.}\times s^{\beta}$$

$$+\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\gamma-\alpha}\times\overline{\gamma-\beta}\times\overline{\gamma-\delta}\times\overline{\gamma-\varepsilon}\times\&c.}\times s^{\gamma}+\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\varepsilon}\times\&c.}{\overline{\delta-\alpha}\times\overline{\delta-\beta}\times\overline{\delta-\gamma}\times\overline{\delta-\varepsilon}\times\&c.}\times s^{\delta}$$

$$+\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\&c.}{\overline{\varepsilon-\alpha}\times\overline{\varepsilon-\beta}\times\overline{\varepsilon-\gamma}\times\overline{\varepsilon-\delta}\times\&c.}\times s^{\varepsilon}+\&c.$$

DEMONSTRATION.

Write α for x in the equation $y=$

$$\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}\times s^{\alpha}+\frac{\overline{x-\alpha}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\beta-\alpha}\times\overline{\beta-\gamma}\times\overline{\beta-\delta}\times\overline{\beta-\varepsilon}\times\&c.}\times s^{\beta}+$$

&c.; and all the terms but the firſt in the reſulting equation will vaniſh, for each of them contains in its numerator a factor $x-\alpha=\alpha-\alpha=0$; and the equation will become $y=\frac{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}\times s^{\alpha}=s^{\alpha}$. In the ſame manner, by writing β, γ, δ, ε, &c. ſucceſſively for x in the given equation it may be proved, that when x is equal to β, γ, δ, ε, &c. then will y become reſpectively s^{β}, s^{γ}, s^{δ}, s^{ε}, which was to be demonſtrated.

2. Aſſume $y=ax^r+bx^{r+s}+cx^{r+2s}+dx^{r+3s} \ldots x^{r+\overline{n-1}s}$; and when x becomes α, β, γ, δ, ε, &c. let y become reſpectively

ſpectively s^{α}, s^{β}, s^{γ}, s^{δ}, s^{ε}, &c.; then will $y =$

$$\frac{x^r \times \overline{x^s - \beta^s} \times \overline{x^s - \gamma^s} \times \overline{x^s - \delta^s} \times \overline{x^s - \varepsilon^s} \times \&c.}{\alpha^r \times \overline{\alpha^s - \beta^s} \times \overline{\alpha^s - \gamma^s} \times \overline{\alpha^s - \delta^s} \times \overline{\alpha^s - \varepsilon^s} \times \&c.} \times s^{\alpha}$$

$$+ \frac{x^r \times \overline{x^s - \alpha^s} \times \overline{x^s - \gamma^s} \times \overline{x^s - \delta^s} \times \overline{x^s - \varepsilon^s} \times \&c.}{\beta^r \times \overline{\beta^s - \alpha^s} \times \overline{\beta^s - \gamma^s} \times \overline{\beta^s - \delta^s} \times \overline{\beta^s - \varepsilon^s} \times \&c.} \times s^{\beta}$$

$$+ \frac{x^r \times \overline{x^s - \alpha^s} \times \overline{x^s - \beta^s} \times \overline{x^s - \delta^s} \times \overline{x^s - \varepsilon^s} \times \&c.}{\gamma^r \times \overline{\gamma^s - \alpha^s} \times \overline{\gamma^s - \beta^s} \times \overline{\gamma^s - \delta^s} \times \overline{\gamma^s - \varepsilon^s} \times \&c.} \times s^{\gamma} + \&c..$$

This may be demonſtrated in the ſame manner as the preceding theorem, by writing α, β, γ, δ, ε, &c. ſucceſſively for x.

PROBLEM.

Let there be n values α, β, γ, δ, ε, &c. of the quantity x, to which the n values s^{α}, s^{β}, s^{γ}, s^{δ}, s^{ε}, &c. of the quantity y correſpond; ſuppoſe theſe quantities to be found by any function X of the quantity x; let π, ϱ, σ, τ, &c. be values of the quantities x, to which s^{π}, s^{ϱ}, s^{σ}, s^{τ}, &c. values of the quantity y correſpond: for x ſubſtitute its abovementioned values π, ϱ, σ, τ, &c. in the function X, and let the quantities reſulting be s^{π}, s^{ϱ}, s^{σ}, s^{τ}, &c. not equal to the preceding s^{π}, s^{ϱ}, s^{σ}, s^{τ}, &c. reſpectively; to find a quantity which added to the function X ſhall not only give the true values of the quantity y correſponding to the values α, β, γ, δ, ε, &c. of the quantity x, but alſo cor-

correſponding to the values π, ϱ, σ, τ, &c. of the above-mentioned quantity x.

Aſſume $S^{\pi}-s^{\pi}=T^{\pi}$, $S^{\varrho}-s^{\varrho}=T^{\varrho}$, $S^{\sigma}-s^{\sigma}=T^{\sigma}$, $S^{\tau}-s^{\tau}=T^{\tau}$, &c.; then the errors of the function X will be reſpectively T^{π}, T^{ϱ}, T^{σ}, T^{τ}, &c.; and the correcting quantity ſought may be

$$\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\text{ &c.}}{\overline{\pi-\alpha}\times\overline{\pi-\beta}\times\overline{\pi-\gamma}\times\overline{\pi-\delta}\times\overline{\pi-\varepsilon}\times\text{ &c.}}\times\frac{\overline{x-\varrho}\times\overline{x-\sigma}\times\overline{x-\tau}\times\text{ &c.}}{\overline{\pi-\varrho}\times\overline{\pi-\sigma}\times\overline{\pi-\tau}\times\text{ &c.}}\times T^{\pi}$$

$$+\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\text{ &c.}}{\overline{\varrho-\alpha}\times\overline{\varrho-\beta}\times\overline{\varrho-\gamma}\times\overline{\varrho-\delta}\times\overline{\varrho-\varepsilon}\times\text{ &c.}}\times\frac{\overline{x-\pi}\times\overline{x-\sigma}\times\overline{x-\tau}\times\text{ &c.}}{\overline{\varrho-\pi}\times\overline{\varrho-\sigma}\times\overline{\varrho-\tau}\times\text{ &c.}}\times T^{\varrho}$$

$$+\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\text{ &c.}}{\overline{\sigma-\alpha}\times\overline{\sigma-\beta}\times\overline{\sigma-\gamma}\times\overline{\sigma-\delta}\times\overline{\sigma-\varepsilon}\times\text{ &c.}}\times\frac{\overline{x-\pi}\times\overline{x-\varrho}\times\overline{x-\tau}\times\text{ &c.}}{\overline{\sigma-\pi}\times\overline{\sigma-\varrho}\times\overline{\sigma-\tau}\times\text{ &c.}}\times T^{\sigma}$$

$$+\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\text{ &c.}}{\overline{\tau-\alpha}\times\overline{\tau-\beta}\times\overline{\tau-\gamma}\times\overline{\tau-\delta}\times\overline{\tau-\varepsilon}\times\text{ &c.}}\times\frac{\overline{x-\pi}\times\overline{x-\varrho}\times\overline{x-\sigma}\times\text{ &c.}}{\overline{\tau-\pi}\times\overline{\quad}-\times\overline{\tau-\sigma}\times\text{ &c.}}\times T^{\tau}$$

+ &c.

Aliter.

Let $\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times$ &c. $\times\overline{x-\pi}\times\overline{x-\varrho}\times\overline{x-\sigma}\times\overline{x-\tau}\times$ &c. $=N$; $\overline{\pi-\alpha}\times\overline{\pi-\beta}\times\overline{\pi-\gamma}\times\overline{\pi-\delta}\times\overline{\pi-\varepsilon}\times$ &c. $\times\overline{\pi-\varrho}\times\overline{\pi-\sigma}\times\overline{\pi-\tau}\times$ &c. $=\Pi$; $\overline{\varrho-\alpha}\times\overline{\varrho-\beta}\times\overline{\varrho-\gamma}\times\overline{\varrho-\delta}\times\overline{\varrho-\varepsilon}\times$ &c. $\times\overline{\varrho-\pi}\times\overline{\varrho-\sigma}\times\overline{\varrho-\tau}\times$ &c. $=P$; $\overline{\sigma-\alpha}\times\overline{\sigma-\beta}\times\overline{\sigma-\gamma}\times\overline{\sigma-\delta}\times\overline{\sigma-\varepsilon}\times$ &c. $\times\overline{\sigma-\pi}\times\overline{\sigma-\varrho}\times\overline{\sigma-\tau}\times$ &c. $=\Sigma$; $\overline{\tau-\alpha}\times\overline{\tau-\beta}\times\overline{\tau-\gamma}\times\overline{\tau-\delta}\times\overline{\tau-\varepsilon}\times$ &c. $\times\overline{\tau-\pi}\times\overline{\tau-\varrho}\times\overline{\tau-\sigma}\times$ &c. $=T$, &c.; then may the correcting quantity ſought be $N\left(\frac{T^{\tau}}{\Pi\times\overline{x-\pi}}+\frac{T^{\varrho}}{P\times\overline{x-\varrho}}\right.$

$$+\frac{T^{\sigma}}{\Sigma\times\overline{x-\sigma}}+\frac{T^{\tau}}{T\times\overline{x-\tau}}+\text{ &c.}\Big).$$

This

This problem may be demonſtrated in the ſame manner as the preceding theorems, by writing for x in the correcting quantity ſucceſſively its values π, ϱ, σ, τ, &c.

2. For the correcting quantity ſought may be aſſumed the quantity

$$\frac{x^s-\alpha^s \times x^s-\beta^s \times x^s-\gamma^s \times x^s-\delta^s \times \&c. \times x^r \times x^s-\varrho^s \times x^s-\sigma^s}{\pi^s-\alpha^s \times \pi^s-\beta^s \times \pi^s-\gamma^s \times \pi^s-\delta^s \times \&c. \times \pi^r \times \pi^s-\varrho^s \times \pi^s-\sigma^s} \times \frac{x^s-\tau^s \times \&c.}{\times \pi^s-\tau^s \times \&c.} \times T^\pi + \frac{x^s-\alpha^s \times x^s-\beta^s \times x^s-\gamma^s \times x^s-\delta^s \times \&c. \times x^r \times x^s-\pi^s}{\varrho^s-\alpha^s \times \varrho^s-\beta^s \times \varrho^s-\gamma^s \times \varrho^s-\delta^s \times \&c. \times \varrho^r \times \varrho^s-\pi^s} \times \frac{x^s-\sigma^s \times x^s-\tau^s \times \&c.}{\times \varrho^s-\sigma^s \times \varrho^s-\tau^s \times \&c.} \times T^\varrho + \&c.$$

3. In general, let Z be any quantity which is $=o$, when x becomes either α, β, γ, δ, ε, &c.: let Z become ſucceſſively A, B, C, D, &c. when x becomes π, ϱ, σ, τ, &c. reſpectively. When x either $=\varrho$, σ, τ, &c. let $\Pi=o$; but if $x=\pi$, let $\Pi=p$: in the ſame manner when x either $=\pi$, σ, τ, &c. let $P=o$; but when $x=\varrho$ let $P=r$: and ſimilarly, let $\Sigma=o$ when x is either π, ϱ, τ, &c.; but when $x=\sigma$ let $\Sigma=s$: and likewiſe, when x is either π, ϱ, σ, &c. let $T=o$; but when $x=\tau$ let $T=t$: &c. then for the correcting quantity ſought may be aſſumed $\frac{Z}{A} \times \frac{\Pi}{p} \times T^\pi + \frac{Z}{B} \times \frac{P}{r} \times T^\varrho + \frac{Z}{C} \times \frac{\Sigma}{s} \times T^\sigma + \frac{Z}{D} \times \frac{T}{t} \times T^\tau + \&c.$

THE-

THEOREM.

Assume (n) quantities $\alpha, \beta, \gamma, \delta, \varepsilon$, &c. then will the sum of all the (n) quantities of the following kind

$$\frac{\alpha^m}{\alpha-\beta\times\alpha-\gamma\times\alpha-\delta\times\alpha-\varepsilon\times\&c.}+\frac{\beta^m}{\beta-\alpha\times\beta-\gamma\times\beta-\delta\times\beta-\varepsilon\times\&c.}$$

$$+\frac{\gamma^m}{\gamma-\alpha\times\gamma-\beta\times\gamma-\delta\times\gamma-\varepsilon\times\&c.}+\frac{\delta^m}{\delta-\alpha\times\delta-\beta\times\delta-\gamma\times\delta-\varepsilon\times\&c.}$$

$$+\frac{\varepsilon^m}{\varepsilon-\alpha\times\varepsilon-\beta\times\varepsilon-\gamma\times\varepsilon-\delta\times\&c.}+\&c.=0,$$ if m be any whole number less than $n-1$; but if $m=n-1$, then will the above mentioned sum $=1$. In general, the sum of the n terms $\frac{\alpha^m(\beta\gamma\delta\,\&c.+\beta\gamma\varepsilon\,\&c.+\beta\delta\varepsilon+\gamma\delta\varepsilon\,\&c.+\&c.)}{\alpha-\beta\times\alpha-\gamma\times\alpha-\delta\times\alpha-\varepsilon\times\&c.}+$

$$\frac{\beta^m(\alpha\gamma\delta\,\&c.+\alpha\gamma\varepsilon\,\&c.+\alpha\delta\varepsilon\,\&c.+\gamma\delta\varepsilon\,\&c.+\&c.)}{\beta-\alpha\times\beta-\gamma\times\beta-\delta\times\beta-\varepsilon\times\&c.}+$$

$$\frac{\gamma^m(\alpha\beta\delta\,\&c.+\alpha\beta\varepsilon\,\&c.+\alpha\delta\varepsilon\,\&c.+\beta\delta\varepsilon\,\&c.+\&c.)}{\gamma-\alpha\times\gamma-\beta\times\gamma-\delta\times\gamma-\varepsilon\,\&c.}+$$

$$\frac{\delta^m(\alpha\beta\gamma\,\&c.+\alpha\beta\varepsilon\,\&c.+\alpha\gamma\varepsilon\,\&c.+\&c.)}{\delta-\alpha\times\delta-\beta\times\delta-\gamma\times\delta-\varepsilon\times\&c.}+$$

$$\frac{\varepsilon^m(\alpha\beta\gamma\,\&c.+\alpha\beta\delta\,\&c.+\alpha\gamma\delta\,\&c.+\beta\gamma\delta\,\&c.+\&c.)}{\varepsilon-\alpha\times\varepsilon-\beta\times\varepsilon-\gamma\times\varepsilon-\delta\times\&c.}+\&c.=0,$$

if m be less than n, and $m+r$ not equal to $n-1$, where r is equal to the number of letters contained in each of the contents above mentioned $\beta\gamma\delta$, &c. $\beta\gamma\varepsilon$, &c. $\beta\delta\varepsilon$, &c. $\gamma\delta\varepsilon$, &c. &c. &c. respectively: but if $m+r=n-1$, then will the above mentioned sum $=\pm 1$; it will be $+1$ if r be an even number, otherwise -1.

DEMONSTRATION.

Suppoſe $a+b\alpha+c\alpha^2+d\alpha^3+e\alpha^4+$ &c. $=s^{\alpha}$,

$a+b\beta+c\beta^2+d\beta^3+e\beta^4+$ &c. $=s^{\beta}$,

$a+b\gamma+c\gamma^2+d\gamma^3+e\gamma^4+$ &c. $=s^{\gamma}$,

$a+b\delta+c\delta^2+d\delta^3+e\delta^4+$ &c. $=s^{\delta}$,

$a+b\varepsilon+c\varepsilon^2+d\varepsilon^3+e\varepsilon^4+$ &c. $=s^{\varepsilon}$, multiply theſe equations into A, B, C, D, E, &c. unknown co-efficients to be inveſtigated, and there reſult

$A\times s^{\alpha}=Aa+Ab\alpha+Ac\alpha^2+Ad\alpha^3+Ae\alpha^4+$&c.

$B\times s^{\beta}=Ba+Bb\beta+Bc\beta^2+Bd\beta^3+Be\beta^4+$&c.

$C\times s^{\gamma}=Ca+Cb\gamma+Cc\gamma^2+Cd\gamma^3+Cd\gamma^4+$&c.

$D\times s^{\delta}=Da+Db\delta+Dc\delta^2+Dd\delta^3+Dd\delta^4+$&c.

$E\times s^{\varepsilon}=Ea+Eb\varepsilon+Ec\varepsilon^2+Ed\varepsilon^3+Ee\varepsilon^4+$&c. &c. &c.

Now ſuppoſe $As^{\alpha}+Bs^{\beta}+Cs^{\gamma}+Ds^{\delta}+Es^{\varepsilon}+$ &c. $=a+bx+cx^2+dx^3+ex^4+$&c. and the correſpondent parts reſpectively equal to each other; that is, $a(A+B+C+D+E+$ &c.$)=a$; $b(A\alpha+B\beta+C\gamma+D\delta+E\varepsilon+$&c.$)=bx$; $A\alpha^2+B\beta^2+C\gamma^2+D\delta^2+E\varepsilon^2+$ &c. $=x^2$; $A\alpha^3+B\beta^3+C\gamma^3+D\delta^3+E\varepsilon^3+$ &c. $=x^3$; $A\alpha^4+B\beta^4+C\gamma^4+D\delta^4+E\varepsilon^4+$ &c. $=x^4$, &c.: But it follows from Theorem I. that (if $As^{\alpha}+Bs^{\beta}+Cs^{\gamma}+Ds^{\delta}+Es^{\varepsilon}+$&c. $=a+bx+cx^2+dx^3+ex^4+$&c.) $A=\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}$,

THEOREM.

Assume (n) quantities $\alpha, \beta, \gamma, \delta, \varepsilon$, &c. then will the sum of all the (n) quantities of the following kind

$$\frac{\alpha^m}{\alpha-\beta\times\alpha-\gamma\times\alpha-\delta\times\alpha-\varepsilon\times\&c.} + \frac{\beta^m}{\beta-\alpha\times\beta-\gamma\times\beta-\delta\times\beta-\varepsilon\times\&c.}$$

$$+ \frac{\gamma^m}{\gamma-\alpha\times\gamma-\beta\times\gamma-\delta\times\gamma-\varepsilon\times\&c.} + \frac{\delta^m}{\delta-\alpha\times\delta-\beta\times\delta-\gamma\times\delta-\varepsilon\times\&c.}$$

$$+ \frac{\varepsilon^m}{\varepsilon-\alpha\times\varepsilon-\beta\times\varepsilon-\gamma\times\varepsilon-\delta\times\&c.} + \&c. = 0,$$ if m be any whole number less than $n-1$; but if $m=n-1$, then will the above mentioned sum $=1$. In general, the sum of the n terms $\frac{\alpha^m(\beta\gamma\delta\,\&c.+\beta\gamma\varepsilon\,\&c.+\beta\delta\varepsilon+\gamma\delta\varepsilon\,\&c.+\&c.)}{\alpha-\beta\times\alpha-\gamma\times\alpha-\delta\times\alpha-\varepsilon\times\&c.} +$

$$\frac{\beta^m(\alpha\gamma\delta\,\&c.+\alpha\gamma\varepsilon\,\&c.+\alpha\delta\varepsilon\,\&c.+\gamma\delta\varepsilon\,\&c.+\&c.)}{\beta-\alpha\times\beta-\gamma\times\beta-\delta\times\beta-\varepsilon\times\&c.} +$$

$$\frac{\gamma^m(\alpha\beta\delta\,\&c.+\alpha\beta\varepsilon\,\&c.+\alpha\delta\varepsilon\,\&c.+\beta\delta\varepsilon\,\&c.+\&c.)}{\gamma-\alpha\times\gamma-\beta\times\gamma-\delta\times\gamma-\varepsilon\,\&c.} +$$

$$\frac{\delta^m(\alpha\beta\gamma\,\&c.+\alpha\beta\varepsilon\,\&c.+\alpha\gamma\varepsilon\,\&c.+\&c.)}{\delta-\alpha\times\delta-\beta\times\delta-\gamma\times\delta-\varepsilon\times\&c.} +$$

$$\frac{\varepsilon^m(\alpha\beta\gamma\,\&c.+\alpha\beta\delta\,\&c.+\alpha\gamma\delta\,\&c.+\beta\gamma\delta\,\&c.+\&c.)}{\varepsilon-\alpha\times\varepsilon-\beta\times\varepsilon-\gamma\times\varepsilon-\delta\times\&c.} + \&c. = 0,$$

if m be less than n, and $m+r$ not equal to $n-1$, where r is equal to the number of letters contained in each of the contents above mentioned $\beta\gamma\delta$, &c. $\beta\gamma\varepsilon$, &c. $\beta\delta\varepsilon$, &c. $\gamma\delta\varepsilon$, &c. &c. &c. respectively: but if $m+r=n-1$, then will the above mentioned sum $=\pm 1$; it will be $+1$ if r be an even number, otherwise -1.

DEMONSTRATION.

Suppoſe $a+b\alpha+c\alpha^2+d\alpha^3+e\alpha^4+$ &c. $=s^{\alpha}$,

$a+b\beta+c\beta^2+d\beta^3+e\beta^4+$ &c. $=s^{\beta}$,

$a+b\gamma+c\gamma^2+d\gamma^3+e\gamma^4+$ &c. $=s^{\gamma}$,

$a+b\delta+c\delta^2+d\delta^3+e\delta^4+$ &c. $=s^{\delta}$,

$a+b\varepsilon+c\varepsilon^2+d\varepsilon^3+e\varepsilon^4+$ &c. $=s^{\varepsilon}$, multiply theſe equations into A, B, C, D, E, &c. unknown co-efficients to be inveſtigated, and there reſult

$A\times s^{\alpha}=Aa+Ab\alpha+Ac\alpha^2+Ad\alpha^3+Ae\alpha^4+$&c.

$B\times s^{\beta}=Ba+Bb\beta+Bc\beta^2+Bd\beta^3+Be\beta^4+$&c.

$C\times s^{\gamma}=Ca+Cb\gamma+Cc\gamma^2+Cd\gamma^3+Cd\gamma^4+$&c.

$D\times s^{\delta}=Da+Db\delta+Dc\delta^2+Dd\delta^3+Dd\delta^4+$&c.

$E\times s^{\varepsilon}=Ea+Eb\varepsilon+Ec\varepsilon^2+Ed\varepsilon^3+Ee\varepsilon^4+$&c. &c. &c.

Now ſuppoſe $As^{\alpha}+Bs^{\beta}+Cs^{\gamma}+Ds^{\delta}+Es^{\varepsilon}+$ &c. $=a+bx+cx^2+dx^3+ex^4+$&c. and the correſpondent parts reſpectively equal to each other; that is, $a(A+B+C+D+E+$ &c.$)=a$; $b(A\alpha+B\beta+C\gamma+D\delta+E\varepsilon+$&c.$)=bx$; $A\alpha^2+B\beta^2+C\gamma^2+D\delta^2+E\varepsilon^2+$ &c. $=x^2$; $A\alpha^3+B\beta^3+C\gamma^3+D\delta^3+E\varepsilon^3+$ &c. $=x^3$; $A\alpha^4+B\beta^4+C\gamma^4+D\delta^4+E\varepsilon^4+$ &c. $=x^4$, &c.: But it follows from Theorem I. that (if $As^{\alpha}+Bs^{\beta}+Cs^{\gamma}+Ds^{\delta}+Es^{\varepsilon}+$&c. $=a+bx+cx^2+dx^3+ex^4+$&c.) $A=\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}$,

$$B=\frac{\overline{x-\alpha}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\beta-\alpha}\times\overline{\beta-\gamma}\times\overline{\beta-\delta}\times\overline{\beta-\varepsilon}\times\&c.},\quad C=\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c}{\overline{\gamma-\alpha}\times\overline{\gamma-\beta}\times\overline{\gamma-\delta}\times\overline{\gamma-\varepsilon}\times\&c.},$$

$$D=\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\varepsilon}\times\&c.}{\overline{\delta-\alpha}\times\overline{\delta-\beta}\times\overline{\delta-\gamma}\times\overline{\delta-\varepsilon}\times\&c.},\quad E=\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\&c.}{\overline{\varepsilon-\alpha}\times\overline{\varepsilon-\beta}\times\overline{\varepsilon-\gamma}\times\overline{\varepsilon-\delta}\times\&c.},$$

&c.: ſubſtitute theſe values for A, B, C, D, E, &c. reſpectively in the preceding equations ($A+B+C+D+E+\&c.=1$, $A\alpha+B\beta+C\gamma+D\delta+E\varepsilon+\&c.=x$, $A\alpha^2+B\beta^2+C\gamma^2+D\delta^2+E\varepsilon^2+\&c.=x^2$, $A\alpha^3+B\beta^3+C\gamma^3+D\delta^3+E\varepsilon^3+\&c.=x^3$, &c.) and there reſult the equations (1) $\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}$

$$+\frac{\overline{x-\alpha}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\beta-\alpha}\times\overline{\beta-\gamma}\times\overline{\beta-\delta}\times\overline{\beta-\varepsilon}\times\&c.}+\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\gamma-\alpha}\times\overline{\gamma-\beta}\times\overline{\gamma-\delta}\times\overline{\gamma-\varepsilon}\times\&e.}+\&c.=1;$$

$$(2)\,\alpha\times\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}+\beta\times\frac{\overline{x-\alpha}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\beta-\alpha}\times\overline{\beta-\gamma}\times\overline{\beta-\delta}\times\overline{\beta-\varepsilon}\times\&c.}$$

$$+\gamma\times\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\gamma-\alpha}\times\overline{\gamma-\beta}\times\overline{\gamma-\delta}\times\overline{\gamma-\varepsilon}\times\&c.}+\&c.=x;$$

$$(3)\,\alpha^2\times\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}+\beta^2\times\frac{\overline{x-\alpha}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\beta-\alpha}\times\overline{\beta-\gamma}\times\overline{\beta-\delta}\times\overline{\beta-\varepsilon}\times\&c.}$$

$$+\gamma^2\times\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\gamma-\alpha}\times\overline{\gamma-\beta}\times\overline{\gamma-\delta}\times\overline{\gamma-\varepsilon}\times\&c.}=x^2;$$ and in general,

$$\alpha^m\times\frac{\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\alpha-\beta}\times\overline{\alpha-\gamma}\times\overline{\alpha-\delta}\times\overline{\alpha-\varepsilon}\times\&c.}+\beta^m\times\frac{\overline{x-\alpha}\times\overline{x-\gamma}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\beta-\alpha}\times\overline{\beta-\gamma}\times\overline{\beta-\delta}\times\overline{\beta-\varepsilon}\times\&c.}$$

$$+\gamma^m\times\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\delta}\times\overline{x-\varepsilon}\times\&c.}{\overline{\gamma-\alpha}\times\overline{\gamma-\beta}\times\overline{\gamma-\delta}\times\overline{\gamma-\varepsilon}\times\&c.}+\delta^m\times\frac{\overline{x-\alpha}\times\overline{x-\beta}\times\overline{x-\gamma}\times\overline{x-\varepsilon}\times\&c.}{\overline{\delta-\alpha}\times\overline{\delta-\beta}\times\overline{\delta-\gamma}\times\overline{\delta-\varepsilon}\times\&c.}$$

$+\&c.=x^m$, whatever may be the values of the quantities x; α, β, γ, δ, ε, &c.: reduce all theſe fractions into terms, proceeding according to the dimenſions of the quantity x, and it is evident, that the ſum of all the fractions multiplied

tiplied into any dimenſion of x not equal to m will be $= 0$; but the ſum of all the fractions multiplied into x^m will be $= 1$: from this propoſition the theorem is eaſily deduced.

I have invented and demonſtrated from different principles to the preceding the firſt part of this theorem, a particular caſe of which was publiſhed by me many years ago.

From this theorem may eaſily be deduced ſeveral others of a ſimilar nature.

VIII. *Diſquiſitio de tempore Periodico Cometæ anno* 1770 *obſervati. Autore* J. A. Lexell, *Academiæ Scientiarum Petrop. Socio. Communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read Feb. 21, 1778.

TEMPORIBUS periodicis cometarum inveſtigandis, quum aſtronomi nondum ſuas impenderint curas, quod hæc aſtra intervallo temporis, quo incolis terræ conſpicua eſſe ſolent, exiguas valde portiones ſuarum orbitarum conficiant, ita ut exinde vix quicquam certi, de excentricitatibus iſtarum orbitarum concludi queat; inexſpectatum omnino aſtronomis contigit, quod tempus periodicum cometæ anno 1770 conſpicui ex obſervationibus concludere poſſe mihi viſus ſim, mirum autem et vix ulla fide dignum videri debuit, quod hoc tempus periodicum adeo exiguum a me inventum eſt, ut vix quinque annos cum dimidio ſuperaret, ita ut hic cometa ſuas periodos circa ſolem minori adhuc tempore, quam Jupiter et Saturnus abſolvere deberet. Propoſitum quidem mihi nunc non eſt, omnium calculorum, quibus ad hanc concluſionem perductus ſum, adumbrationem hic tradere, quippe quod prolixum valde et pro inſti-

tuto

tuto minus neceſſarium foret; ſufficit ut argumentum proponam pro iſta hypotheſi temporis periodici ſtabilienda, meo quidem judicio ita ſtringens, ut demonſtrationi geometricæ æquiparari queat. Hujus autem argumenti vis, in eo conſiſtit, ut elementis adhibitis, quæ tempori periodico quinque annorum et ſeptem menſium, conformia ſunt, obſervationibus hujus cometæ optime ſatisfieri demonſtretur, contra vero ſi tempus periodicum cometæ majus ſupponatur, inſignes et vix quidem probabiles errores obſervationibus induci. Sunt igitur elementa, pro motu cometæ a me ſtabilita, ſequentia:

1. Longitudo nodi aſcendentis $4^s\ 12°\ 0'$.

2. Inclinatio orbitæ ad eclipticam $1°\ 33'\ 40''$.

3. Elongatio nodi deſcendentis a perihelio $44°\ 17'\ 4''$, ideoque longitudo perihelii $11^s\ 26°\ 16'\ 26''$.

4. Tempus tranſitus per perihelium anno 1770 die 13 Aug. $13^h\ 5'$ circiter, ſive 13,5450 Aug.

5. Cometæ diſtantia perihelii 0,6743815, cujus log. = 9,8289057.

6. Semiaxis orbitæ a cometa deſcriptæ = 3,1478606, cujus log. = 0,4980155. Hinc log. ſemiparametri = 0,0807300, et log. excentric. = 9,8952927, ideoque tempus periodicum 5,585 annorum.

His elementis adhibitis, ſequentes comparationes locorum cometæ ex theoria deductorum, cum obſervatis, habebuntur.

	Temp. med. Paris.	Long. com. obſer.	Long. com. ex calculo.	Diff.	Latit. com. obſervat.	Latit. com. ex calculo.	Diff.
	h ′ ″	☉ ° ′ ″	☉ ° ′ ″	′ ″	° ′ ″	° ′ ″	′
Jun. 14	11 29 48	9 2 47 44	9 2 48 1	+0 17	6 40 24B	6 40 54B	+0
15	11 23 22	9 2 51 49	9 2 51 54	+0 5	6 57 15	6 57 51	+0
17	11 11 33	9 2 59 58	9 3 0 52	+0 54	7 38 51	7 38 37	−0
20	10 40 48	9 3 16 12	9 3 17 2	+0 50	9 5 38	9 5 29	−0
21	10 27 45	9 3 23 17	9 3 24 26	+1 9	9 44 36	9 45 27	+0
22	12 9 36	9 3 33 43	9 3 34 9	+0 26	10 39 5	10 39 54	+0
24	12 3 18	9 3 59 58	9 3 59 51	−0 7	12 59 55	12 59 52	−0
25	13 27 55	9 4 21 42	9 4 21 21	−0 21	14 53 56	14 54 37	+0
27	13 13 17	9 5 34 54	9 5 36 6	+1 12	21 8 47	21 8 38	−0
28	10 46 34	9 6 45 58	9 6 43 58	−2 0	26 29 33	26 30 39	+1
29	11 59 26	9 9 42 45	9 9 42 30	−0 15	37 57 32	38 0 37	+3
Aug. 2	15 3 15	3 6 2 32	3 6 2 4	−0 28	0 50 7A	0 49 59A	−0
3	14 45 9	3 6 25 15	3 6 24 32	−0 43	0 53 36	0 52 46	−0
4	14 12 48	3 6 47 24	3 6 47 46	+0 18	0 55 44	0 56 4	+0
5	14 38 43	3 7 13 53	3 7 13 3	−0 48	0 59 17	0 58 50	−0
6	14 29 42	3 7 39 25	3 7 38 45	−0 40	1 1 18	1 1 16	−0
7	14 49 19	3 8 6 40	3 8 5 59	−0 41	1 3 35	1 3 34	−0
8	14 20 13	3 8 33 29	3 8 33 17	−0 12	1 6 36	1 5 35	−1
9	14 48 10	3 9 4 30	3 9 2 40	−1 50	1 9 33	1 7 29	−2
10	14 14 27	3 9 32 36	3 9 31 44	−0 52	1 9 13	1 9 9	−0
11	14 23 23	3 10 3 2	3 10 2 37	−0 25	1 10 51	1 10 40	−0
12	14 46 25	3 10 34 48	3 10 34 43	−0 5	1 12 52	1 12 4	−0
14	14 37 29	3 11 40 25	3 11 40 4	−0 21	1 15 25	1 14 25	−1
15	15 43 33	3 12 14 56	3 12 15 37	+0 41	1 16 43	1 15 26	−1
18	14 24 32	3 13 59 24	3 13 59 37	+0 13	1 18 35	1 17 39	−0
19	14 33 18	3 14 36 3	3 14 36 16	+0 13	1 19 18	1 18 37	−0
26	15 39 38	3 19 3 44	3 19 3 59	+0 15	1 20 45	1 20 5	−0

	Temp. med. Paris.	Long. com. obſerv.	Long. ex calculo.	Diff.	Latit. com. obſervat.	Latit. ex calculo.	Diff.
	h ′ ″	s ° ′ ″	s ° ′ ″	′ ″	° ′ ″	° ′ ″	′ ″
28	14 44 8	3 20 19 42	3 20 20 27	+0 45	1 20 55A	1 20 5A	−0 50
29	15 21 53	3 21 0 3	3 21 0 31	+0 28	1 20 5	1 20 3	−0 2
30	14 48 22	3 21 39 12	3 21 38 44	−0 28	1 20 56	1 19 58	−0 58
31	14 38 25	3 22 17 16	3 22 17 49	+0 33	1 20 23	1 19 50	−0 33
4	15 5 20	3 24 52 41	3 24 53 46	+1 5	1 19 23	1 19 2	−0 21
5	14 48 37	3 25 31 7	3 25 31 44	+0 37	1 19 45	1 18 47	−0 58
8	15 57 4	3 27 26 32	3 27 27 41	+1 9	1 18 23	1 17 51	−0 32
9	15 6 30	3 28 3 19	3 28 3 52	+0 33	1 18 22	1 17 33	−0 49
10	16 26 33	3 28 40 26	3 28 43 15	+2 49	1 19 4	1 17 12	−1 52
14	14 16 26	4 1 4 36	4 1 5 35	+0 59	1 16 18	1 15 46	−0 32
17	15 53 3	4 2 52 40	4 2 53 13	+0 33	1 14 52	1 14 35	−0 17
18	15 30 16	4 3 26 57	4 3 27 6	+0 9	1 14 58	1 14 12	−0 46
19	15 19 4	4 4 0 32	4 4 0 37	+0 5	1 13 2	1 13 48	+0 46
20	15 33 45	4 4 34 17	4 4 34 27	+0 10	1 12 35	1 13 35	+1 0
29	15 23 51	4 9 14 54	4 9 15 53	+0 59	1 10 16	1 9 42	−0 34
1	15 23 22	4 10 12 6	4 10 13 31	+1 25	1 9 51	1 8 54	−0 57
—	16 33 28	4 10 13 54	4 10 14 55	+1 1	1 10 4	1 8 53	−1 11
2	15 43 37	4 10 40 51	4 10 41 59	+1 9	1 10 4	1 8 29	−1 35
—	16 38 50	4 10 41 52	4 10 43 5	+1 13	1 10 10	1 8 29	−1 41

Conſenſus itaque theoriæ cum obſervationibus jam quidem tantus eſt, ut vix majorem deſiderare liceat, nam pro longitudinibus aberratio nunquam duo minuta prima ſupergreditur, niſi pro obſervatione die 10 Septembris inſtituta, quæ tamen obſervatio, uti ex comparatione cum reliquis patefiet, non poteſt non aliquantum eſſe dubia.

Pro-

Pro latitudinibus quoque, errores etiamſi plerumque negativum ſortiantur valorem, majores tamen non ſunt, quam ut veriſimiles videri debeant; nam differentia pro obſervatione diei 29 Junii, quæ eſt 3 minutorum, ope parallaxeos cometæ, quæ tum temporis inſignis erat, facilem admittit explicationem. Cæterum facile tamen intelligitur, elementa obſervationibus ſatisfacientia cum aliqua latitudine aſſumi poſſe. Sic ſi tempus periodicum ſtatuatur 5, 6 annorum, log. ſemiparam. orbitæ = 0,0808000, log. diſt. perih. 9,8288794, tempus perihelii = 13,5400 Aug. long. ☊ = 4^s 12° 9′, inclinatio orbitæ 1° 33′ 40″, elongatio a ☊ = 44° 7′ 59″, erunt loca cometæ ex calculo.

		‸	°	′	″		°	′	″	
Pro 15 Jun.	long.	9	2	52	12	lat.	6	58	37	Bor.
29		9	9	42	59		38	0	54	
2 Aug.		3	6	2	7		38	49	35	A.
29		3	21	0	33		1	20	1	
1 Octob.		4	10	13	20		1	9	0	

Quæ loca æque bene, vel aliquanto melius cum obſervatione conſentiunt, ac quæ ſupra attulimus. Nunc igitur diſpiciamus an tempus periodicum inſignius auctum, elementis inveniendis inſerviat quæ cum obſervationibus componi queant. Supponamus igitur primum omnes obſervationes ad unam eandemque orbitam cometæ pertinere,

tinere, ſeu quod idem eſt, cometam in approximatione ſua ad tellurem, ab actione telluris affectum non fuiſſe; deinde enim viſuri erimus, quid ſtatuendum ſit, ſi ſupponatur actionem telluris in motum cometæ aliquem habuiſſe influxum. Jam igitur aſſumpto certo tempore periodico, uti primum 6 annorum, et adhibito certo valore pro ſemiparametro orbitæ, in reliqua ejus elementa inquiſivi quæ ita eſſent comparata, ut obſervationibus diebus 15 et 29 Junii inſtitutis, ſatisfacerent; patet enim quod ſi bina cometæ elementa, uti tempus periodicum et parameter orbitæ, pro cognitis habeantur, omnia quæ ad motum cometæ ſtabiliendum deſiderantur, ope duarum obſervationum inveſtigari poſſe.

Poſitis itaque nunc, tempore periodico 6 annorum, et log. ſemipar. = 0,0817000, reliqua elementa erunt; log. diſt. perih. = 9.8273218, temp. perih. = 13,2850 Auguſti, long. ☊ = 4ˢ 12° 6′, inclinatio orbitæ = 1° 34′ 30″, elong. perih. a ♈ = 44° 9′ 56″. Hincque loca cometæ ex calculo.

		ˢ	°	′	″		°	′	″	
Pro 15 Jun.	long.	9	2	51	52	lat.	6	58	6	Bor.
29		9	9	43	6		38	0	27	
2 Aug.		3	6	3	18		38	50	15	
29		3	21	5	42		1	20	6	
1 Oct.		4	10	11	54		1	9	10	

Suppoſito iterum tempore periodico 6 annorum et log. ſemipar. = 0,0818500 erit tempus perihelii 13,2900 Aug. long. ☊ = 4ˢ 12° 28′, inclinatio orbitæ = 1° 34′ 2″, elong. perihel. a ♈ = 43° 37′ 28″, tumque erunt:

	Comet. longit.				Latitud.			
	ˢ	°	′	″	°	′	″	
Pro 15 Jun.	9	2	51	25	6	58	6	Bor.
29	9	9	42	42	38	0	24	
2 Aug.	3	6	2	47	0	49	3	Auſt.
29	3	21	4	56	1	19	36	
1 Oct.	4	10	10	59	1	9	1	

Pro hac igitur poſteriori hypotheſi, obſervationibus dierum 2 et 29 Aug. reſpectu longitudinis quidem melius ſatisfit, quam per priorem hypotheſin, ac obſervatio die 1 Octobr. reſpectu longitudinis jam magis evadit erronea, tumque etiam reſpectu latitudinis errores aliquantum augentur; concludi autem hinc quoque poteſt, quæcunque etiam ſuppoſitio fiat pro ſemiparametro orbitæ ſi tempus periodicum ſtatuatur 6 annorum, et obſervationibus diebus 15 et 29 Junii ſatisfaciendum ſit, obſervationes occurrere, quæ erroribus ſaltem duorum minutorum, tam negativis quam poſitivis afficientur. Quod ſi jam ſupponatur tempus periodicum 7 annorum et log. ſemiparam. = 0,0837000, erit tempus perihelii 12,7950 Auguſt. long. ☊ = 4ˢ 12° 49′, inclinatio orbitæ

= 1° 35′ 30″, elongatio perihelii a ☊ = 43° 26′ 10″, tumque habebuntur:

	Com. longit.					Latitudo.			
	˙	°	′	″		°	′	″	
Pro 15 Jun.	9	2	52	1		6	58	16	Bor.
29	9	9	42	50		38	0	28	
2 Aug.	3	6	6	19		38	49	3	Auſt.
29	5	21	16	18		1	19	31	
1 Oct.	4	10	9	22		1	9	17	

Deinde adhibito iterum tempore periodico 7 annorum et log. ſemipar. = 0,0840000, erit tempus perihelii = 12,8050 Aug. long. ☊ = 4ˢ 13° 58′, inclinatio orbitæ = 1° 33′ 50″, et long. perih. a ♈ = 42° 14′ 41″, hincque ſequuntur.

		˙	°	′	″		°	′	″	
Pro 15 Jun.	long.	9	2	51	54	lat.	6	58	30	B.
29		9	9	42	38		38	0	30	
2 Aug.		3	6	5	59		38	45	25	A.
29		3	21	13	54		1	17	57	
1 Oct.		4	10	6	2		1	9	13	

Poſteriori adhibita hypotheſi, errores obſervationum die 2 et 29 Aug. inſtitutarum, quoad longitudinem aliquantum minuuntur, in latitudinem autem tanto majores, redundant, tumque obſervatio die 1 Oct. inſtituta ſecundum poſteriorem hypotheſin multo magis redditur erronea. Certum igitur eſt cum tempore periodico ſeptem

annorum omnes obſervationes circa cometam anni 1770 nulla ratione impleri poſſe; quin potius adeo enormes errores in nonnullas earum redundare, ut vix ullam invenire queant fidem. Reſtat autem nunc, ut examinemus, an ne ſaltem omnibus obſervationibus a die 2 Auguſti uſque ad 2 Octobris inſtitutis, ſatisfieri queat; adhibito tempore periodico aliquantum majori. Et tum quidem evidens eſt, quia arcus cometæ circa ſolem deſcriptus a 2 Auguſti uſque ad 2 Octobris, multo minor eſt illo, quem a 15 Jun. ad 2 Octobris percurrit, tempus periodicum quo obſervationibus ſecundæ apparitionis ſatisfaciendum ſit, jam cum majori latitudine aſſumi poſſe, quam ſi omnibus in univerſum obſervationibus ſatisfaciendum eſſet. Hinc igitur ſi tempus periodicum ſtatuetur 6 annorum, ejuſmodi quidem elementa facile inveniri poſſunt, ut obſervationes a 2 Aug. ad 2 Octobris factæ, ſaltem abſque gravioribus erroribus, impleantur. Diſpiciamus itaque quid evenire debeat, ſi tempus periodicum ſtatuatur 7 annorum. Generatim igitur quum compertum mihi fuerit, latitudinibus cometæ facile ſatisfieri, modo longitudinibus fuerit ſatisfactum, operam dedi, ut tribus cometæ longitudinibus obſervatis ſatisfacerem; tumque examinavi qualis error in quartam aliquam obſervationem redundaret. Quatuor autem obſervationes, quarum ubique uſum feci ſunt illa, quæ diebus 2, 12, 29 Aug. et 1 Oct.

inſtitutæ habentur. Primum igitur elementa inveſtigavi, quibus obſervationes dierum 2, 29 Auguſt. et 1 Octobris implentur, quæ ſequentia mihi ſe obtulerunt.

Log. ſemipar. = 0,0925000, tempus perih. = 15,6280 Auguſt, elong. perih. a ♉ = 46° 14′ 6″, poſita longitudine ☊ = 4ˢ 12° 0′, loca autem cometæ ex theoria deducta, nunc ita ſe habebunt:

Longitudo cometæ.

	ˢ	°	′	″
Pro 2 Aug.	3	6	2	27
12	3	10	41	45
29	3	21	0	20
1 Oct.	4	10	12	29

Ubi in obſervatione die 12 Auguſti facta reperitur error 7 minutorum primorum. Deinde aliis calculis comperi, quod ſi ſub hypotheſi temporis periodici 7 annorum obſervationes dierum 2 Aug. et 1 Octobris impleantur, quicunque demum error in obſervatione die 29 Auguſti admittatur, errorem obſervationis die 12 Auguſti factæ, nunquam infra 7 minuta prima deprimi poſſe; unde tanto magis concludere licet, aucto tempore periodico, obſervationes dierum 2, 12 Aug. et 1 Octob. multo minus inter ſe componi poſſe. Ulterius pergendo, operam dedi ut obſervationes diebus 12 et 29 Aug. atque 1 Oct. factas implerem, quod ſequentibus elementis obtinui.

Log.

Log. ſemi-par. = 0,0915000, temp. perih. = 15,1340 Aug. elong. perih. a ♈ = 45° 59′ 24″, poſita longitudine nodi ut ſupra, tum enim erant:

		Longitud. cometæ.			
		‛	°	′	″
Pro	2 Aug.	3	5	49	1
	12	3	10	34	41
	29	3	21	0	20
	1 Oct.	4	10	12	6

Ubi obſervatio die 2 Auguſti facta, 13 minutis primis redditur erronea, qui etiam minimus fere error eſt, quem hæc obſervatio admittit, dum obſervationes dierum 12 Aug. et 1 Octob. ſub hypotheſi temporis periodici ſeptem annorum implendæ ſunt. Deinde ſi idem adhibeatur tempus periodicum, et obſervationibus dierum 2, 12, et 29 Aug. ſatisfactum ſit, quod fiet ponenda log. ſemipar. = 0,0837600, tempus perih. = 12,1500 Auguſt, elongat. a ♈ = 43° 16′ 33″, longit. nodi ut ſupra, in obſervatione die 1 Octobris inſtituta, reperietur error 35 min. prim. Omnes autem hæ diſquiſitiones eo tendunt, ut perficiatur, obſervationes a 2 Aug. uſque ad 2 Octob. inſtitutas, ſub hypotheſi temporis periodici 7 annorum nunquam perfecte impleri poſſe, ſed inter illas ſaltem nonnullas occurrere, quæ erroribus 7 minut. primorum obnoxiæ ſunt, quod quidem vix ulla veriſimilitudine

tudine gaudet. Quamvis hinc facile concludi poſſit, aucta quantitate temporis periodici, errores obſervationum augeri, ipſemet tamen calculo inſtituto ea de re certior fieri volui. Poſito itaque tempore periodico 8 annor. log. ſemipar. = 0,1000000, tempore perih. = 17,2300, elong. perih. a ♈ = 47° 32′ 4″, long. ☊ ut ſupra adhibita ſequentes inveni longitudines cometæ.

		°	′	″
Pro 2 Aug.	3	6	2	44
12	3	10	47	42
15	3	12	26	33
29	3	21	0	57
1 Oct.	4	10	12	6

Ubi jam in obſervatione die 12 Aug. inſtituto, occurrit error 13 minutorum primorum. Tum vero me non monente intelligitur, quod ſuppoſita tempore periodico ſeptem annorum, quæcunque demum elementa adhibeantur inter illa, quæ obſervationibus ſecundæ apparitionis ſatisfacere debent, inde enormes omnino errores pro obſervationibus primæ apparitionis emergere. Operæ quidem pretium non eſſe judicavi, ut inquirerem, an valor temporis periodici allatus ſenſibilem admittat diminutionem, quia hoc unicuique minus probabile videbitur; ex iis autem, quæ jam attuli, facile colligitur, ſi omnibus in univerſum obſervationibus ſatisfaciendum ſit, tempus

periodicum

periodicum allatum vix dimidia parte anni infra illum valorem, quem ſuppoſui, deprimi poſſe; ſin autem obſervationibus ſecundæ apparitionis ſolummodo ſatisfaciendum ſit, omnes quidem valores temporis periodici intra limites 5 et 6 annorum contenti, abſque metu ſenſibilis erroris in obſervationes redundantis admitti poſſent. Exiſtimaverim tamen hos limites nullo modo ultra quatuor annos cum dimidio et 6 annos cum dimidio prorogari poſſe, quin potius maxime mihi eſt vero ſimile, valorem temporis periodici, arctioribus iſtis limitibus 5, vel 6 annorum circumſcribi.

Quo autem certius eſt argumentum; quod pro ſtabiliendo tempore periodico cometæ anno 1770 obſervati, jam propoſui, eo ſane magis unicuique mirum videri debet, quod hunc eundem cometam non niſi unica vice obſervare licuerit. Nam ſi hic cometa ſingulis quinque annis et ſeptem menſibus ad ſuum rediret perihelium, utique fieri debuiſſe videtur, ut ſaltem hoc ſeculo, poſtquam ſtudium cœlorum majori aſſiduitate cœli cœptum eſt, ſæpius ſe conſpiciendum præbuiſſet. Cum variæ quidem conjecturæ proponi poſſent pro explicando eventu adeo ſingulari, tum inter illas eam adferre, quæ maxima probabilitate ſe commendat, hac occaſione ſufficiet. Quum diſtantia aphelia cometæ a ſole, diſtantiam Jovis ab hoc aſtro fere exæquet, ſtatim ſuſpicio quædam ſuboritur,

ſuboritur, an non fieri potuit, ut per actionem Jovis, motus cometæ olim fuerit perturbatus, ita ut cometa hic antehac orbitam, a præſenti multo diverſam, deſcripſerit. Calculo autem inſtituto reperitur, cometam fuiſſe in conjunctione cum Jove, Anno 1767, die 27 Maii, eorumque diſtantiam tum temporis fuiſſe 58vam partem diſtantiæ cometæ a ſole, unde habito reſpectu maſſarum Solis et Jovis, colligitur actionem Jovis in cometam fuiſſe triplo majorem actione Solis, ideoque ſenſibilem omnino effectum ad motum cometæ perturbandum producere valuiſſe; eo potius quod in aphelio cometa motu admodum lento feratur, adeoque ſatis diu actioni Jovis fuerit expoſitus. Ulterius per elementa ſupra a nobis ſtabilita colligitur, proxime futuram conjunctionem Jovis cum cometa contingere debere anno proxime ſequenti die 23 Auguſti, exiſtente tunc diſtantia cometæ a Jove non niſi 491^ma^ parte diſtantiæ cometæ a Sole, ideoque actionem Jovis in cometam actione Solis 224vicibus majorem eſſe, unde totalis mutatio motus cometæ non poterit non oriri. Cæterum hæ concluſiones pro exacte veris haberi non poterunt, niſi quatenus elementa pro motu cometæ allata, fuerint exactiſſime ſtabilita; quippe quum leviſſima mutatio horum elementorum, imprimiſque temporis periodici, diſtantiam cometæ apheliam ſenſibili mutatione afficiat, et per facilem calculum exploravi

quod ſi tempus periodicum cometæ aliquantulum diminuatur, eo effici ut actio Jovis in priori conjunctione augeatur, contra vero in poſteriori conjunctione diminuatur. Hoc igitur ratiocinio id præ-primis mihi fuit propoſitum, ut oſtenderem, fieri utique potuiſſe, quod hic cometa ob actionem Jovis coactus fuerit ejuſmodi orbitam deſcribere, quæ ex obſervationibus anno 1770 factis colligitur, licet antea motum ſuum perfecerit in orbita, cujus tempus periodicum ſatis inſigne eſſe potuerit. Utrum cometa noſter in approximatione ſua ad tellurem, ab actione telluris fuerit affectus, id quidem nec affirmare nec negare auſim, ſaltem veriſimile mihi videtur, hanc actionem non admodum ſenſibiles mutationes producere valuiſſe, et ex ſuperioribus quidem conſtat, pro tempore periodico cometæ non admodum magnas alterationes oriri potuiſſe.

Denique licet valde dubium eſſe queat, utrum cometa noſter in proximo ad perihelium acceſſu, nobis ſe conſpiciendum præbeat, quod motus ejus ab actione Jovis totalem forſan ſubierit mutationem; tamen aſtronomis haud prorſus ingratum eſſe exiſtimaverim, ſi tabulam ſubjunxerim, quæ oſtendet in quibuſnam cœli regionibus ſingulis menſibus cometam hunc quærere oporteat, reſpectu quoque habito ad majores vel minores valores temporis periodici, limitibus 5 et 6 annorum contentos.

	Long.	Latit.	Long.	Latit.	Long	Latit.	Long.	Latit.	Long.	Latit.
	s o	o ′	s o	o ′	s o	o ′	s ′	o ′	s o	o ′
aii 1	0 13*	0 14A	11 12	0 49B	10 5	2 7B	8 27	2 55B		
15	0 5	0 20B	10 26	2 6	9 1	3 29	7 29	3 13		
n. 1	1 3	0 18A	11 27	1 33	9 3	4 41	7 22	3 25		
15	1 0	0 30B	9 4	7 7	7 12	3 18	7 3*	2 38		
l. 1	1 26	0 25A	in conj. cum ☉		7 5	2 56	7 0*	2 17		
15	in conj. cum ☉		6 29	2 29	6 29*	1 59				
g. 1	3 5	0 49A	6 24	1 37	7 1*	1 36				
15	3 12	1 16	6 23	0 51	7 4*	1 17				
pt. 1	3 23	1 19	6 1	0 58A	7 10	0 50				
15	4 1	1 15	5 8	2 10	7 16	0 27				
ct. 1	4 10	1 9	4 22	2 27	7 24	0 31A				
15	4 21	2 13	7 27	1 34						
ov. 1	4 22	1 58	7 14	5 8	8 22	1 5				
15	4 20	1 52	6 25	6 1	9 6	0 54				
ec. 1	4 10	1 37	4 15	7 44	10 4	5 42	10 20	2 55A		
15	4 0	1 20	3 22	4 31	9 3	19 7	10 29	3 14	9 28	0 39A
an. 1	3 5	2 36	0 13	3 12	10 28	1 16A				
15	3 0	1 42	1 10	2 19	11 23	1 32	10 18*	0 31		
eb. 1	3 1	1 2	2 5	1 0	0 23	1 26	11 19	0 57		
15	3 4*	0 40	0 10*	1 2	11 8*	0 28	10 14*	0 9B		
ar. 1	0 3*	0 44	11 6*	0 12	10 12*	0 28B				
15	0 24*	0 49	11 24*	0 29	11 1*	0 10				
pr. 1	11 25*	0 12	10 28*	0 33B	10 1*	1 21				
15	0 16*	0 29	11 18*	0 12	10 17*	1 13				

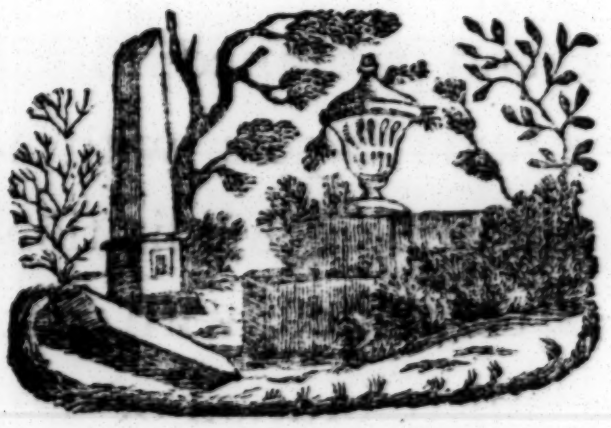
De hac tabula notari convenit, quod in illa ſaltus non nunquam majores prodeant, quod inde evenit, quia ex una columna verticali in præcedentem tranſitus factus ſit;

ſic ſi ſupponatur tempus perihelii incidere in 13 Junii, erit pro 1 Junii longitudo cometæ 11ˢ 27° et latitudo 1° 33′, quæ in columna verticali ſecunda allegantur; pro 15 autem Junii, erit longitudo 1ˢ 0° et latitudo 30′ bor. quæ in columna verticali prima afferuntur. Fundamentum autem cui conſtructio hujus tabulæ innititur, in eo poſitum eſt, quod ſuppoſuerim cometam ſi incolis telluris conſpicuus evadat, non multo magis a terra debere eſſe remotum, ac erat dum anno 1770 menſe Octobris conſpicuus eſſe deſiit, hincque etiam loca in tabula noſtra aſteriſcis notata indicant, dubium eſſe, an cometa in his locis viſibilis fiat.

IN a pamphlet of eighteen pages in quarto, publiſhed at Upſal in 1776, Mr. ERIC PROSPERIN, member of the Royal Academies of Sciences at Stockholm and Upſal, and Aſtronomer to the king of Sweden, has ſhewn by his calculations, that the obſervations of near four months made on this comet by M. MESSIER could not be repreſented by a parabolic orbit; and founds a ſtrong conjecture thereon, and on the circumſtances of the different parabolas which he found neceſſary to repreſent the motion of the comet at different periods of time during its

appearance, that its orbit may be ſenſibly elliptical (which it ſeems M. PINGRE, who firſt calculated the orbit in a parabola, had alſo ſome ſuſpicion of) and concludes with recommending the inveſtigation of the true elements of its orbit in an ellipſis. The laborious calculation thus recommended has, we ſee, been ſince ſucceſſfully and ſatisfactorily performed in this paper by Mr LEXELL.

N. MASKELYNE.



IX. *On the General Reſolution of Algebraical Equations.* *by* Edward Waring, *M. D. F. R. S. and of the Inſtitute of* Bononia, *Lucaſian Profeſſor of Mathematics in the Univerſity of* Cambridge.

Read Jan. 28, 1779.

IN the year 1757 I ſent ſome papers to the Royal Society, which papers were printed in the year 1759, and copies of them delivered to ſeveral perſons; theſe papers ſomewhat corrected, with the addition of a ſecond part on the properties of curve lines, were publiſhed in the year 1762. In the years 1767, 1768 and 1769 I printed, and publiſhed in the beginning of the year 1770, the ſame papers with additions and emendations under the title of Meditationes Algebraicæ. In theſe papers were contained, with many other inventions, the moſt general reſolution of algebraical equations known, as it contains the reſolution of every algebraical equation, of which the general reſolution has been given, viz. the reſolution of quadratic, cubic and biquadratic, the reſolution of Mr. DE MOIVRE's and Mr. BEROUT's (ſince publiſhed) equations; it diſcovers the reſolution

reſolution of an equation of n dimenſions, of which the n roots are given, and alſo deduces innumerable equations of n dimenſions, which contain $n-1$ independent coefficients. From whence it ſeems probable, that this new method of mine may contain the moſt general reſolution of algebraical equations that ever has, or, perhaps, ever will be invented.

The general reſolution is $x = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + d\sqrt[n]{p^4} \ldots + r\sqrt[n]{p^{n-3}} + s\sqrt[n]{p^{n-2}} + t\sqrt[n]{p^{n-1}} + \frac{A}{n}$, if the equation be $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3} + Dx^{n-4} - \&c. = 0$.

I ſhall add the reſolution of ſome particular equations from this method, and then ſubjoin the equation to which $x = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \&c.$ is the general reſolution.

1. Let the reſolution be $x = a\sqrt[3]{p} + b\sqrt[3]{p^2}$, and the correſpondent equation free from radicals will be found $x^3 - 3abpx - a^3p - b^3p^2 = 0$. Let $x^3 - Px - Q = 0$ be a cubic equation whoſe reſolution is required, which ſuppoſe the ſame as the equation found above, and conſequently their correſpondent terms equal, i. e. $P = 3abp$ and $Q = a^3p + b^3p^2$, whence $p = \frac{P}{3ab}$, which value being ſubſtituted for p in the ſecond equation, there reſults $Q = \frac{Pa^2}{3b} + \frac{bP^2}{9a^2}$. In this equation for a or b may be aſſumed unity, or any other

other quantity whatever, and there will result an equation of the formula of a quadratic from which the other b or a may be found, whence from the equation ($p = \frac{P}{3ab}$) p may be deduced, and consequently the resolution of the cubic required.

In the same manner for p may be assumed any quantity whatever, and in the equation $Q = a^3p + b^3p^2$ for b substitute its value $\frac{P}{3ap}$, or for a its value $\frac{P}{3bp}$, and there result the equations $Q = a^3p + \frac{P^3}{27a^3p}$, and $Q = \frac{P^3}{27b^3p} + b^3p^2$, which have the formula of a quadratic, from which may be deduced the resolution of the cubic required.

2. Let the resolution assumed be $x = a\sqrt[4]{p} + b\sqrt[4]{p^2} + c\sqrt[4]{p^3}$; exterminate the irrational quantities, and there results the equation $x^4 - (2b^2 + 4ac)px^2 - 4(a^2bp + bc^2p^2)x - a^4p + b^4p^2 - c^4p^3 + 2a^2c^2p^2 - 4ab^2cp^2 = 0$; suppose $p = 1$, and the given equation $x^4 + qx^2 - rx + s = 0$, let the correspondent terms of the given and resulting equations be respectively made equal to each other, and there result the three equations $2b^2 + 4ac = -q$, $4b(a^2 + c^2) = r$, and $a^4 - b^4 + c^4 - 2a^2c^2 + 4ab^2c = -s$; reduce these equations into one, so that the unknown quantities a and c may be exterminated, and there results the equation $4b^6 + 2qb^4 + (\frac{q^2}{4} - s)b^2 - \frac{r^2}{16} = 0$ of the formula of a cubic, from which

the

the unknown quantity b may be found, which being ſubſtituted for its value (b) in the preceding equations, from the equations thence enſuing may be found the unknown quantities a and c, and conſequently the reſolution of the given biquadratic $x^4+qx^2-rx+s=0$.

From the ſame principles can be deduced different reſolutions of the above-mentioned biquadratic $x^4+qx^2-rx+s=0$.

3. I. Let $x=a\sqrt[2n]{p}+b\sqrt[2n]{p^2}$, then will the equation free from radicals be $x^{2n}-2b^npx^n-\frac{n}{1.2}.2nb^{n-1}a^2px^{n-1}-\frac{n\times\overline{n^2-1}}{1.2.3.4}\times 2nb^{n-2}a^4px^{n-2}-\frac{n\times\overline{n^2-1}\times\overline{n^2-4}}{1.2.3.4.5.6}\times 2nb^{n-3}a^6px^{n-3}-\frac{n.\overline{n^2-1}.\overline{n^2-4}.\overline{n^2-9}}{1.2.3.4.5.6.7.8}\times 2nb^{n-4}a^8px^{n-4}\ldots\ldots\ldots-\frac{n.\overline{n^2-1}\times\overline{n^2-4}\times\overline{n^2-9}.\overline{n^2-16}\ldots\overline{n^2-n-2}^2}{1.2.3.4.5.6.7\ldots\ldots 2n-2}\times 2na^{2n-2}bpx=a^np-b^np^2$.

This equation may be deduced from the following principles. Let α, β, γ, δ, ε, &c. be the $2n$ roots of the equation $z^{2n}-1=0$, then (by Prop. XXIII. of my Meditat. Algebraicæ) the equation free from radicals will be the product of the following quantities $(x-a\alpha\sqrt[2n]{p}-b\alpha^2\sqrt[2n]{p^2})(x-a\beta\sqrt[2n]{p}-b\beta^2\sqrt[2n]{p^2})(x-a\gamma\sqrt[2n]{p}-b\gamma^2\sqrt[2n]{p^2})(x-a\delta\sqrt[2n]{p}-b\delta^2\sqrt[2n]{p^2})(x-a\varepsilon\sqrt[2n]{p}-b\varepsilon^2\sqrt[2n]{p^2})$ &c. $=0$: multiply theſe quantities into each other, and from the reſulting product, by Prob. III. of the Meditat. Algebr. eaſily can

be deduced the equation free from radicals which was to be found.

3. II. Let $x=a\sqrt[2n+1]{p}+b\sqrt[2n+1]{p^2}$, then will the correſpondent equation free from radicals be $x^{2n+1}-\overline{2n+1}\,b^n apx^n-\frac{n.n+1}{1.2.3}\times\overline{2n+1}\,b^{n-1}a^3px^{n-1}-\frac{n\times\overline{n^2-1}\times\overline{n+2}}{1.2.3.4.5}\times\overline{2n+1}\,b^{n-2}a^5px^{n-2}-\frac{n\times\overline{n^2-1}\times\overline{n^2-4}\times\overline{n+3}}{1.2.3.4.5.6.7}\times\overline{2n+1}\,b^{n-3}a^7px^{n-3}-\frac{n.n^2-1.n^2-4.n^2-9.n+4}{1.2.3.4.5.6.7.8.9}\times\overline{2n+1}\,b^{n-4}a^9px^{n-4}+\ldots.-\frac{n.n^2-1.n^2-4.n^2-9.n^2-16\ldots n^2-n-2^2\times 2n-1}{1.2.3.4.5.6.7.8.9\ldots.\overline{2n-1}}\times\overline{2n+1}\,ba^{2n-1}px=a^{2n+1}p+b^{2n+1}p^2$.

This may be derived from the ſame principles as the preceding.

3. III. In general let the equation be $x=a\sqrt[m]{p}+b\sqrt[m]{p^2}$, then will the equation free from radicals become $x^m-ma^{m-2}bpx-m.\frac{m-3}{2}a^{m-4}b^2px^2-m.\frac{m-4}{2}.\frac{m-5}{3}a^{m-6}b^3px^3-m.\frac{m-5}{2}.\frac{m-6}{3}.\frac{m-7}{4}a^{m-8}b^4px^4-m.\frac{m-6}{2}.\frac{m-7}{3}.\frac{m-8}{4}.\frac{m-9}{5}a^{m-10}b^5px^5-$ &c. $=a^mp\pm b^mp^2$; if m denotes an even number, it will be $-b^mp^2$, but if an odd number, it will be $+b^mp^2$.

4. I. Let n denote an odd number, and $x=a\sqrt[n]{p}+b\sqrt[n]{p^3}$, then will $x^n-p\,(na^{n-3}bx^2+n.\frac{n-5}{2}a^{n-6}b^2x^4+n.\frac{n-7}{2}.\frac{n-8}{3}a^{n-9}b^3x^5+n.\frac{n-9}{2}.\frac{n-10}{3}.\frac{n-11}{4}a^{n-12}b^4x^8+n.\frac{n-11}{2}.\frac{n-12}{3}.\frac{n-13}{4}.\frac{n-14}{5}a^{n-15}b^5x^{10}+n.\frac{n-13}{2}.\frac{n-14}{3}.\frac{n-15}{4}.\frac{n-16}{5}.\frac{n-17}{6}a^{n-18}b^6x^{12}+n.$

$n-$

$\frac{n-15}{2}\cdot\frac{n-16}{3}\cdot\frac{n-17}{4}\cdot\frac{n-18}{5}\cdot\frac{n-19}{6}\cdot\frac{n-20}{7}\,a^{n-21}b^7x^{14}$ + &c.) $\pm p^2$ $(na^{\frac{n-3}{2}}$

$b^{\frac{n+1}{2}}x - \frac{1}{2^2}\times n.\frac{n-5}{2}\cdot\frac{n-7}{3}\,a^{\frac{n-9}{2}}b^{\frac{n+3}{2}}x^3 + \frac{1}{2^4}\times n.\frac{n-7}{2}\cdot\frac{n-9}{3}\cdot\frac{n-11}{4}\cdot\frac{n-13}{5}$

$a^{\frac{n-15}{2}}b^{\frac{n+5}{2}}x^5 - \frac{1}{2^6}\times n.\frac{n-9}{2}\cdot\frac{n-11}{3}\cdot\frac{n-13}{4}\cdot\frac{n-15}{5}\cdot\frac{n-17}{6}\cdot\frac{n-19}{7}\,a^{\frac{n-21}{2}}b^{\frac{n+7}{2}}x^7 +$

$\frac{1}{2^8}\times n.\frac{n-11}{2}\cdot\frac{n-13}{3}\cdot\frac{n-15}{4}\cdot\frac{n-17}{5}\cdot\frac{n-19}{6}\cdot\frac{n-21}{7}\cdot\frac{n-23}{8}\cdot\frac{n-25}{9}\,a^{\frac{n-27}{2}}b^{\frac{n+9}{2}}x^9 -$

&c.) $= a^np + b^np^3$.

The quantity $\pm p^2$ denotes $+p^2$ if $\frac{n-3}{4}$ is a whole number, otherwiſe $-p^2$.

4. II. Let n denote an even number, and x as before $= a\sqrt[n]{p} + b\sqrt[n]{p^3}$, then will $x^n - p$ $(na^{n-3}bx^2 + n.\frac{n-5}{2}\,a^{n-6}b^2x^4$

$+ n.\frac{n-7}{2}\cdot\frac{n-8}{3}\,a^{n-9}b^3x^6 + n.\frac{n-9}{2}\cdot\frac{n-10}{3}\cdot\frac{n-11}{4}\,a^{n-12}b^4x^8 + n.\frac{n-11}{2}\cdot$

$\frac{n-12}{3}\cdot\frac{n-13}{4}\cdot\frac{n-14}{5}\,a^{n-15}b^5x^{10}$ + &c.) $\pm p^2$ $(\frac{1}{2}n.\frac{n-4}{2}\,a^{\frac{n-6}{2}}b^{\frac{n+2}{2}}x^2 -$

$\frac{1}{2^3}\times n.\frac{n-6}{2}\cdot\frac{n-8}{3}\cdot\frac{n-10}{4}\,a^{\frac{n-12}{2}}b^{\frac{n+4}{2}}x^4 + \frac{1}{2^5}\times n.\frac{n-8}{2}\cdot\frac{n-10}{3}\cdot\frac{n-12}{4}\cdot\frac{n-14}{5}\cdot$

$\frac{n-16}{6}\,a^{\frac{n-18}{2}}b^{\frac{n+6}{2}}x^6 - \frac{1}{2^7}\times n.\frac{n-10}{2}\cdot\frac{n-12}{3}\cdot\frac{n-14}{4}\cdot\frac{n-16}{5}\cdot\frac{n-18}{6}\cdot\frac{n-20}{7}\cdot\frac{n-22}{8}$

$a^{\frac{n-24}{2}}b^{\frac{n+8}{2}}x^8$ + &c.) $= a^np + b^np^3 \pm 2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$.

The quantities $\pm p^2$ and $\pm 2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$ denote $+p^2$, and $+2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$, if $\frac{n-2}{4}$ is a whole number, otherwiſe they denote $-p^2$ and $-2a^{\frac{n}{2}}b^{\frac{n}{2}}p^2$ reſpectively.

5. I. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-1}}$, and n an odd number,

then will $x^n - nabpx^{n-2} + n.\frac{n-3}{2}a^2b^2p^2x^{n-4} - n.\frac{n-4}{2}.\frac{n-5}{3}$ $a^3b^3p^3x^{n-6} + n.\frac{n-5}{2}\frac{n-6}{3}.\frac{n-7}{4}a^4b^4p^4x^{n-8} - \&c. = a^np + b^np^{n-1}$.

5. II. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-1}}$, and n an even number, then will $x^n - nabpx^{n-2} + n.\frac{n-3}{2}a^2b^2p^2x^{n-4} - n.\frac{n-4}{2}.$ $\frac{n-5}{3}a^3b^3p^3x^{n-6} + n.\frac{n-5}{2}.\frac{n-6}{3}.\frac{n-7}{4}a^4b^4p^4x^{n-8} - \&c. = a^np \mp$ $2a^{\frac{n}{2}}b^{\frac{n}{2}}p^{\frac{n}{2}} + b^np^{n-1}$; it will be $+ 2a^{\frac{n}{2}}b^{\frac{n}{2}}p^{\frac{n}{2}}$ if $n = 4r + 2$; but $- 2a^{\frac{n}{2}}b^{\frac{n}{2}}p^{\frac{n}{2}}$ if $n = 4r$.

6. I. Let $x = a\sqrt[n]{p} + b\sqrt[n]{p^{n-2}}$, and n an odd number, which has not the number 3 for a diviſor, then will $x^n - na^2bpx^{n-3} + n.\frac{n-5}{2}a^4b^2p^2x^{n-6} - n.\frac{n-7}{2}.\frac{n-8}{3}a^6b^3p^3x^{n-9} +$ $n.\frac{n-9}{2}.\frac{n-10}{3}.\frac{n-11}{4}a^8b^4p^4x^{n-12} - n.\frac{n-11}{2}.\frac{n-12}{3}.\frac{n-13}{4}.\frac{n-14}{5}$ $a^{10}b^5p^5x^{n-15} + n.\frac{n-13}{2}.\frac{n\times 14}{3}.\frac{n-15}{4}.\frac{n-16}{5}.\frac{n-17}{6}a^{12}b^6p^6x^{n-6} - \&c.$ (to m terms, where m is the number either equal to, or the leaſt greater than $\frac{n}{3}$) $- ab^{\frac{n+1}{2}}p^{\frac{n-1}{2}}(nx^{\frac{n-3}{2}} + \frac{1}{4}n.\frac{n-5}{2}.\frac{n-7}{3}$ $a^2bpx^{\frac{n-9}{2}} + \frac{1}{2^4}n.\frac{n-7}{2}.\frac{n-9}{3}.\frac{n-11}{4}.\frac{n-13}{5}a^4b^2p^2x^{\frac{n-15}{2}} + \frac{1}{2^6}\times n.\frac{n-9}{2}.$ $\frac{n-11}{3}.\frac{n-13}{4}.\frac{n-15}{5}.\frac{n-17}{6}.\frac{n-19}{7}a^6b^3p^3x^{\frac{n-21}{2}} + \frac{1}{2^8}\times n.\frac{n-11}{2}.\frac{n-13}{3}.\frac{n-15}{4}.$ $\frac{n-17}{5}.\frac{n-19}{6}.\frac{n-21}{7}.\frac{n-23}{8}.\frac{n-25}{9}a^8b^4p^4x^{\frac{n-27}{2}} + \&c.) = A = a^np +$ b^np^{n-2}.

Let n be an odd number diviſible by 3, then will the above-

above-mentioned quantity $= A = a^n p + b^n p^{n-2} + 3a^{\frac{n}{3}} b^{\frac{2n}{3}} p^{\frac{2n}{3}-1} + 3a^{\frac{2n}{3}} b^{\frac{n}{3}} p^{\frac{n}{3}}$.

6. II. Let n be an even number, not diviſible by 3, then will $x^n - na^2bpx^{n-3} + n.\frac{n-5}{2} a^4b^2p^2x^{n-6} - n.\frac{n-7}{2}.\frac{n-8}{3} a^6b^3p^3x^{n-9} + n.\frac{n-9}{2}.\frac{n-10}{3}.\frac{n-11}{4} a^8b^4p^4x^{n-12} - n.\frac{n-11}{2}.\frac{n-12}{3}.\frac{n-13}{4}.\frac{n-14}{5} a^{10}b^5p^5x^{n-15} + n.\frac{n-13}{2}.\frac{n-14}{3}.\frac{n-15}{4}.\frac{n-16}{5}.\frac{n-17}{6} a^{12}b^6p^6x^{n-18} -$ &c. to m terms as before $- b^{\frac{n}{2}} p^{\frac{n}{2}-1} (2x^{\frac{n}{2}} + \frac{1}{2} n.\frac{n-4}{2} a^2bpx^{\frac{n}{2}-3} + \frac{1}{2^3} \times n.\frac{n-6}{2}.\frac{n-8}{3}.\frac{n-10}{4} a^4b^2p^2x^{\frac{n}{2}-6} + \frac{1}{2^5} \times n.\frac{n-8}{2}.\frac{n-10}{3}.\frac{n-12}{4}.\frac{n-14}{5}.\frac{n-16}{6} a^6b^3p^3x^{\frac{n}{2}-9} + \frac{1}{2^7} \times n.\frac{n-10}{2}.\frac{n-12}{3}.\frac{n-14}{4}.\frac{n-16}{5}.\frac{n-18}{6}.\frac{n-20}{7}.\frac{n-22}{8} a^8b^4p^4x^{\frac{n}{2}-12} +$ &c.$) = A = a^n p - b^n p^{n-2}$.

Let n be an even number diviſible by 3, then will the above-mentioned quantity $A = a^n p - b^n p^{n-2} - 3a^{\frac{2n}{3}} b^{\frac{n}{3}} p^{\frac{n}{3}} + 3a^{\frac{n}{3}} b^{\frac{2n}{3}} p^{\frac{2n}{3}-1}$.

In all the preceding caſes n, m and r denote whole affirmative numbers.

Theſe equations may be deduced in the ſame manner as is before given in Caſe 3. I; or can be demonſtrated by writing in the equation free from radicals for the different powers of x their values deduced from the given equation $x = a\sqrt[m]{p} + b\sqrt[m]{p^n}$.

To

To render the ſolution general, it may not be improper to ſubjoin the ſubſequent.

LEMMA.

1. Let $\alpha, \beta, \gamma, \delta, \varepsilon, \zeta$, &c. be the reſpective roots of the equation $x^n - 1 = 0$; then will $\alpha^m + \beta^m + \gamma^m + \delta^m + \varepsilon^m + \&c. = 0$, unleſs $n = m$, or n is a diviſor of m, in which caſe $\alpha^m + \beta^m + \gamma^m + \delta^m + \varepsilon^m + \&c. = n$.

2. The ſum of all quantities of the following kind $\alpha^m\beta^r + \alpha^r\beta^m + \alpha^m\gamma^r + \alpha^r\gamma^m + \beta^m\gamma^r + \beta^r\gamma^m + \alpha^m\delta^r + \&c.$ will be $= 0$; unleſs n be either equal to, or a diviſor of $m + r$, in which caſe the ſum above-mentioned will be $= -n$; except n be either equal to m or r, or a diviſor of them, in which caſe the ſum will be $n^2 - n$; but if $m = r$, then in the former caſe will the above-mentioned ſum $= -\frac{n}{2}$, and in the latter $= \frac{n^2 - n}{2}$.

3. The ſum of all quantities of this kind $\alpha^m\beta^r\gamma^s\delta^t \&c. + \alpha^r\beta^m\gamma^s\delta^t \&c. + \alpha^m\beta^r\gamma^t\delta^s \&c. + \alpha^r\beta^s\gamma^m\delta^t \&c. + \&c.$ will be $= 0$, unleſs n be either equal to $r + m + s + t$ &c. or a diviſor of it.

Let π be the number of indices m, r, s, t, &c. and n be either equal to $m + r + s + t + \&c.$ or a diviſor of it, but n be neither equal to, nor a diviſor of the ſum of any two, three, four, ... $\pi - 3$, $\pi - 2$ or $\pi - 1$ of the above-mentioned

mentioned quantities; then will the ſum above-mentioned $= \mp 1.2.3.4...\overline{\pi-2}.\overline{\pi-1} \times n$; where it will be +, if π be an odd number; otherwiſe −.

In this caſe, if a indices be m, b indices be r, c indices be s, d indices be t, &c. then will the above-mentioned ſum $= \mp \frac{1.2\ 3.4...\overline{\pi-2} \times \overline{\pi-1}}{1.2.3..a \times 1.2.3..b \times 1.2.3..c \times 1.2.3..d \times \&c.} \times n$.

Let n be either equal to, or a diviſor of the ſum of any number ϱ (leſs than π) of the above-mentioned quantities m, r, s, t, &c. and conſequently either equal to, or a diviſor of the ſum of the $(\pi-\varrho)$ remaining quantities: find the ſum of all poſſible quantities of this kind $1.2.3..\overline{\varrho-2} \times \overline{\varrho-1} \times 1.2.3..\overline{\pi-\varrho-2} \times \overline{\pi-\varrho-1} \times n^2$, which ſum call A.

Let n be either equal to, or a diviſor of the ſum of any number (σ) of the above-mentioned quantities m, r, s, t, &c; and alſo equal to, or a diviſor of the ſum of any number (ϱ') of the remaining quantities, and conſequently it will be either equal to, or a diviſor of, the ſum of the $(\pi-\varrho'-\sigma)$ remaining quantities; then find the ſum of all poſſible quantities of this ſort $1.2.3..\overline{\sigma-2} \times \overline{\sigma-1} \times 1.2.3..\overline{\varrho'-2} \times \overline{\varrho'-1} \times 1.2.3..\overline{\pi-\varrho'-\sigma-2} \times \overline{\pi-\varrho'-\sigma-1} \times n^3$, which ſum call B.

In the ſame manner let n be either equal to, or a diviſor of the ſum of any number (τ) of the above-mentioned

tioned quantities m, r, s, t, &c; and ſimilarly let n be either equal to, or a diviſor of the ſum of any number (σ') of the remaining quantities; and alſo let n be either equal to, or a diviſor of the ſum of any number (ϱ'') of the remaining quantities; then will n be either equal to, or a diviſor of the ſum of the $(\pi-\tau-\sigma'-\varrho'')$ remaining quantities: find the ſum of all quantities of this ſort $\overline{1.2.3..\tau-2} \times \overline{\tau-1} \times \overline{1.2.3..\sigma'-2} \times \overline{\sigma'-1} \times \overline{1.2.3..\varrho''-2} \times \overline{\varrho''-1} \times \overline{1.2.3..\pi-\tau-\sigma'-\varrho''-2} \times \overline{\pi-\tau-\sigma'-\varrho''-1} \times n^4$, which ſum call c; and ſo on; then will the above-mentioned ſum $\alpha^m\beta^r\gamma^s\delta^t$ &c. $+ \alpha^r\beta^m\gamma^s\delta^t$ &c. $+ \alpha^m\beta^r\gamma^t\delta^s$ &c. $+ \alpha^r\beta^s\gamma^m\delta^t$ &c. + &c. $= \mp(\overline{1.2.3..\pi-2} \times \overline{\pi-1} \times n - A + B - C + D -$ &c.) where it will be + if π be an odd number, otherwiſe −.

In this caſe, if a indices be m, b indices be r, c indices be s, d indices be t, &c. then will the above-mentioned ſum $= \mp \frac{\overline{1.2.3..\pi-2} \times \overline{\pi-1}.n - A + B - C + D -}{1.2.3..a \times 1.2.3..b \times 1.2.3..c \times 1.2.3..d \times}$ &c.

7. Let α, β, γ, δ, ε, &c. be the roots of the equation $z^n - 1 = 0$, and the reſolution be $x = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + d\sqrt[n]{p^4} \ldots\ldots + b\sqrt[n]{p^\lambda} \ldots + k\sqrt[n]{p^\mu} \ldots\ldots + l\sqrt[n]{p^\nu} \ldots + q\sqrt[n]{p^\xi} \ldots\ldots + r\sqrt[n]{p^\phi} \ldots\ldots + s\sqrt[n]{p^{n-4}} + t\sqrt[n]{p^{n-3}} + v\sqrt[n]{p^{n-2}} + u\sqrt[n]{p^{n-1}}$; then will the different values of x be reſpectively $a\sqrt[n]{p} \times \alpha + b\sqrt[n]{p^2} \times \alpha^2 + c\sqrt[n]{p^3} \times \alpha^3 + d\sqrt[n]{p^4} \times \alpha^4 \ldots\ldots + b\sqrt[n]{p^\lambda} \times \alpha^\lambda \ldots + k\sqrt[n]{p^\mu} \times \alpha^\mu \ldots\ldots + s\sqrt[n]{p^{n-4}} \times \alpha^{n-4} + t\sqrt[n]{p^{n-3}} \times \alpha^{n-3} + v\sqrt[n]{p^{n-2}} \times \alpha^{n-2} + u\sqrt[n]{p^{n-1}} \times \alpha^{n-1}$;

$a\sqrt$

$a\sqrt[n]{p}\times\beta+b\sqrt[n]{p^2}\times\beta^2+c\sqrt[n]{p^3}\times\beta^3+d\sqrt[n]{p^4}\times\beta^4\ldots+b\sqrt[n]{p^\lambda}\times\beta^\lambda\ldots+k\sqrt[n]{p^\mu}\times\beta^\mu\ldots\ldots+s\sqrt[n]{p^{n-4}}\times\beta^{n-4}+t\sqrt[n]{p^{n-3}}\times\beta^{n-3}+v\sqrt[n]{p^{n-2}}\times\beta^{n-2}+u\sqrt[n]{p^{n-1}}\times\beta^{n-1}$;

$a\sqrt[n]{p}\times\gamma+b\sqrt[n]{p^2}\times\gamma^2+c\sqrt[n]{p^3}\times\gamma^3+d\sqrt[n]{p^4}\times\gamma^4\ldots+b\sqrt[n]{p^\lambda}\times\gamma^\lambda\ldots+k\sqrt[n]{p^\mu}\times\gamma^\mu\ldots\ldots+s\sqrt[n]{p^{n-4}}\times\gamma^{n-4}+t\sqrt[n]{p^{n-3}}\times\gamma^{n-3}+v\sqrt[n]{p^{n-2}}\times\gamma^{n-2}+u\sqrt[n]{p^{n-1}}\times\gamma^{n-1}$;

$a\sqrt[n]{p}\times\delta+b\sqrt[n]{p^2}\times\delta^2+c\sqrt[n]{p^3}\times\delta^3+d\sqrt[n]{p^4}\times\delta^4\ldots+b\sqrt[n]{p^\lambda}\times\delta^\lambda\ldots+k\sqrt[n]{p^\mu}\times\delta^\mu\ldots\ldots+s\sqrt[n]{p^{n-4}}\times\delta^{n-4}+t\sqrt[n]{p^{n-3}}\times\delta^{n-3}+v\sqrt[n]{p^{n-2}}\times\delta^{n-2}+u\sqrt[n]{p^{n-1}}\times\delta^{n-1}$;

&c. &c. &c. &c.

and conſequently the ſum of the values or roots, which is the coefficient of the ſecond term of the equation ſought, will be $a\sqrt[n]{p}\times\overline{\alpha+\beta+\gamma+\delta+\&c.}\ (o)+b\sqrt[n]{p^2}\times\overline{\alpha^2+\beta^2+\gamma^2+\delta^2+\&c.}\ (o)+c\sqrt[n]{p^3}\times\overline{\alpha^3+\beta^3+\gamma^3+\delta^3+\&c.}\ (o)+\ldots.+v\sqrt[n]{p^{n-2}}\times\overline{\alpha^{n-2}+\beta^{n-2}+\gamma^{n-2}+\delta^{n-2}+\&c.}\ (o)+u\sqrt[n]{p^{n-1}}\times\overline{\alpha^{n-1}+\beta^{n-1}+\gamma^{n-1}+\delta^{n-1}+\&c.}\ (o)=0$.

The ſum of the products of every two of the values or roots, which is the coefficient of the third term of the equation ſought, will be $a^2\sqrt[n]{p^2}\times\overline{\alpha\beta+\alpha\gamma+\beta\gamma+\alpha\delta+\beta\delta+\gamma\delta+\&c.}\ (o)+ab\sqrt[n]{p^3}\times\overline{\alpha\beta^2+\beta\alpha^2+\alpha\gamma^2+\gamma\alpha^2+\beta\gamma^2+\gamma\beta^2+\alpha\delta^2+\delta\alpha^2+\&c.}\ (o)$, and in general all the terms will be 0, unleſs $a\times u\times p\times\overline{\alpha\beta^{n-1}+\beta\alpha^{n-1}+\alpha\gamma^{n-1}+\gamma\alpha^{n-1}+\beta\gamma^{n-1}+}$

$\overline{\gamma\beta^{n-1}+\alpha\delta^{n-1}+\delta\alpha^{n-1}+\beta\delta^{n-1}+\delta\beta^{n-1}+\gamma\delta^{n-1}+\delta\gamma^{n-1}+\&c.}(-n)$
$+bxvxpx\overline{\alpha^2\beta^{n-2}+\beta^2\alpha^{n-2}+\alpha^2\gamma^{n-2}+\gamma^2\alpha^{n-2}+\beta^2\gamma^{n-2}+\gamma^2\beta^{n-2}}$
$\overline{+\alpha^2\delta^{n-2}+\delta^2\alpha^{n-2}+\beta^2\delta^{n-2}+\delta^2\beta^{n-2}+\&c.}(-n)+ctpx$
$\overline{\alpha^3\beta^{n-3}+\beta^3\alpha^{n-3}+\alpha^3\gamma^{n-3}+\gamma^3\alpha^{n-3}+\beta^3\gamma^{n-3}+\gamma^3\beta^{n-3}+\alpha^3\beta^{n-3}+}$
$\overline{\delta^3\alpha^{n-3}+\beta^3\delta^{n-3}+\delta^3\beta^{n-3}+\&c.}(-n)+dspx\overline{\alpha^4\beta^{n-4}+\beta^4\alpha^{n-4}+}$
$\overline{\alpha^4\gamma^{n-4}+\gamma^4\alpha^{n-4}\beta^4\gamma^{n-4}+\gamma^4\beta^{n-4}+\&c.}(-n)+\&c.=-np(au+$
$bv+ct+ds+\&c.)$

If $n=2\lambda$, then will the coefficient of h^2p be $\frac{n}{2}$, i.e. the above-mentioned coefficient will be $-np\,(au+bv+ct+ds+\ldots.+\frac{1}{2}h^2)$.

The ſum of the contents of every three of the above-mentioned values or roots, which is the coefficient of the fourth term of the equation required, will be $a^3\sqrt[n]{p^3}\times\overline{\alpha\beta\gamma+\alpha\beta\delta+\alpha\gamma\delta+\beta\gamma\delta+\&c.}(o)+a^2b\sqrt[n]{p^4}\times$
$\overline{\alpha\beta\gamma^2+\alpha\gamma\beta^2+\beta\gamma\alpha^2+\alpha\beta\delta^2+\&c.}(o)+\&c.+a^2v\sqrt[n]{p^n}\times$
$\overline{\alpha\beta\gamma^{n-2}+\alpha\gamma\beta^{n-2}+\beta\gamma\alpha^{n-2}+\alpha\beta\delta^{n-2}+\alpha\delta\beta^{n-}+\&c.}(\frac{1.2.n}{1.2})+$
$abt\sqrt[n]{p^n}\times\overline{\alpha\beta^2\gamma^{n-3}+\alpha\gamma^2\beta^{n-3}+\beta\alpha^2\gamma^{n-3}+\beta\gamma^2\alpha^{n-3}+\gamma\alpha^2\beta^{n-3}+}$
$\overline{\gamma\beta^2\alpha^{n-3}+\alpha\beta^2\delta^{n-3}\&c.}(1.2.n)+\&c$; and in general all the terms (unleſs the quantity $\sqrt[n]{p^\theta}$ contained in the term have this formula $\sqrt[n]{p^n}=p$, or $\sqrt[n]{p^{2n}}=p^2$) will be $=o$; let the general term be denoted by $hlk\sqrt[n]{p^{\lambda+\mu+\nu}}\times$
$\overline{\alpha^\lambda\beta^\mu\gamma^\nu+\alpha^\lambda\beta^\nu\gamma^\mu+\alpha^\mu\beta^\lambda\gamma^\nu+\alpha^\mu\beta^\nu\gamma^\lambda+\alpha^\nu\beta^\mu\gamma^\lambda+\alpha^\nu\beta^\lambda\gamma^\mu+\alpha^\lambda\beta^\mu\delta^\nu+\&c.}$
firſt let $\lambda+\mu+\nu$ neither be equal to n or $2n$, then will the term

term above-mentioned = 0; if it be equal to n or $2n$, then will the term be $1.2 \times n \times hklp$ or $1.2nhklp^2$.

If two of the three indexes λ, μ, ν be equal to each other, then divide the above-mentioned term by 1.2; if the three indexes be equal, i. e. $\lambda=\mu=\nu$, divide it by 1.2.3: find all quantities of this kind where $\lambda+\mu+\nu$ either is equal to n or $2n$, and add all the term from thence derived, and call the ſum of them A.

The ſum of the contents of every four of the values or roots above-mentioned, which is the coefficient of the fourth term of the equation required, will be $a^{4\,n}\sqrt{p^4} \times \overline{\alpha\beta\gamma\delta+\alpha\beta\gamma\delta} + \&c.\ (o) + a^3 b\sqrt[n]{p^5} \times \overline{\alpha\beta\gamma\delta^2+\alpha\beta\gamma^2\delta+\alpha\beta^2\gamma\delta+\alpha^2\beta\gamma\delta} + \&c.\ (o) + \&c$: let $bklq\sqrt{p^{\lambda+\mu+\nu+\xi}} \times \overline{\alpha^\lambda\beta^\mu\gamma^\nu\delta^\xi+\alpha^\lambda\beta^\mu\gamma^\xi\delta^\nu+\alpha^\lambda\beta^\nu\gamma^\mu\delta^\xi+\alpha^\lambda\beta^\nu\gamma^\xi\delta^\mu+\alpha^\lambda\beta^\xi\gamma^\nu\delta^\mu+\alpha^\lambda\beta^\xi\gamma^\mu\delta^\nu+\alpha^\mu\beta^\lambda\gamma^\nu\delta^\xi} + \&c.$ denote a general term; this term will be = o, unleſs $\lambda+\mu+\nu+\xi$ either $=n$ or $2n$ or $3n$; in which caſe the term will be either $-1.2.3nhklqp$ or $-1.2.3nhklqp^2$ or $-1.2.3nhklqp^3$; unleſs $\lambda+\mu=\nu+\xi=n$, when the above-mentioned term will be $-(1.2.3n-n^2)\,hklqp^2$; in this caſe if $\lambda=\nu$, and conſequently $\mu=\xi$, then it will be $-(1.2.3n-1.2n^2)\,hklqp^2$; but if $\lambda=\mu=\nu=\xi=\frac{n}{2}$, then will the term be $-(1.2.3n-3n^2)\,hklqp^2$.

In all theſe caſes, if two of the indexes λ, μ, ν, ξ be equal, then muſt the term given above be divided by

1.2; if three, by 1.2.3; if four, by 1.2.3.4; and laſtly if two are equal to each other, and the two remaining indexes equal to each other, but not to the former two, then muſt the term aforeſaid be divided by 1.2.1.2.

Find the ſum of all the poſſible terms of this kind, which call B.

In the ſame manner from the preceding Lemma may be found the aggregates of the contents of every five, ſix, ſeven, &c. roots or values multiplied into each other, which call reſpectively C, D, E, &c; then will the equation required be $x^n * - np(au+bv+ct+ds+\&c.)\,x^{n-2} - Ax^{n-3} + Bx^{n-4} - Cx^{n-5} + Dx^{n-6} - \&c. = 0$.

From the ſame principles may be deduced the moſt general reduction yet known of equations to others of inferior dimenſions, *e. g.*

Let (X) $x^n + (A + a\sqrt[m]{p} + b\sqrt[m]{p^2} + c\sqrt[m]{p^3} + \ldots + s\sqrt[m]{p^{m-2}} + t\sqrt[m]{p^{m-1}})\,x^{n-1} + (B + a'\sqrt[m]{p} + b'\sqrt[m]{p^2} + \ldots + s'\sqrt[m]{p^{m-2}} + t'\sqrt[m]{p^{m-1}})\,x^{n-2} + (C + a''\sqrt[m]{p} + b''\sqrt[m]{p^2} + \&c.)\,x^{n-3} + \&c. = 0$; let α, β, γ, δ, &c. be the reſpective roots of the equation $z^m - 1 = 0$, then, from the principles before given, may be formed the different values of the equation X, which being multiplied into each other from the propoſitions beforementioned of the Meditationes Algebraicæ, may be deduced an equation of nm dimenſions free from radicals,

whoſe root is x, and which contains mn unknown quantities A, a, b, c, &c. B, a', b', c', &c. C, a'', b'', c'', & p: for one, two or more of theſe unknown quantities may be aſſumed any quantities whatever, and thence may be deduced equations of mn dimenſions, which may be reduced to equations $x^n+(A+a\sqrt[m]{p}+b\sqrt[m]{p^2}+c\sqrt[m]{p^3}+\&c.)$ $x^{n-1}+\&c.=0$ of n dimenſions.

In the ſame manner may be aſſumed equations, which involve $\sqrt[m]{p}$, $\sqrt[m]{p^2}$,..., $\sqrt[m]{p^{m-1}}$; $\sqrt[r]{Q}$, $\sqrt[r]{Q^2}$, $\sqrt[r]{Q^3}$,...$\sqrt[r]{Q^{r-1}}$; $\sqrt[s]{\tau}$, $\sqrt[s]{\tau^2}$, $\sqrt[s]{\tau^3}$,...., $\sqrt[s]{\tau^{s-1}}$, &c; and from ſo reducing them as to exterminate the irrational quantities, may often be derived equations whoſe reſolutions or reductions are known.

The method of transforming algebraical equations into others, whoſe roots bear any aſſignable algebraical (but not exponential) relation to the roots of a given algebraical equation firſt publiſhed by me in the papers ſent to the Royal Society, and afterwards in the year 1760; and thirdly in my Miſcellanea Analytica; and laſtly in the Meditationes Algebraicæ, and ſince publiſhed by Mr. LE GRANGE in the Berlin Acts, is perhaps (as Mr. LE GRANGE obſerves) more general than Mr. HUDDE's, or any transformation yet invented; it is very uſeful in the reſolution of numerous problems; and

further

further has this peculiar advantage over all other transformations yet invented, that it often eaſily diſcovers ſome of the firſt terms of the equation required, from which many elegant Theorems may be derived.

In the works above-mentioned, viz. Miſcell. Analyt. Medit. Algeb. &c. are given ſome problems ſerving to this transformation; the firſt of which is a ſeries, which from the coefficients of a given algebraical equation ($x^n - px^{n-1} + qx^{n-2} - \&c. = 0$) finds the ſum of any power of the roots (viz. $\alpha^m + \beta^m + \gamma^m + \delta^m + \&c.$ where α, β, γ, δ, &c. denote the roots of the given equation), the law of which ſeries was publiſhed by me many years before that it was given by Mr. EULER. The third Problem often mentioned in this paper is an elegant and uſeful ſeries for finding the ſum of quantities of the following kind, viz. $\alpha^m \beta^r \gamma^s \delta^t$, &c. $+ \alpha^r \beta^m \gamma^s \delta^t$, &c. $+ \alpha^s \beta^m \gamma^r \delta^t$ &c. $+ \alpha^t \beta^m \gamma^r \delta^s$ &c. $+ \alpha^m \beta^t \gamma^r \delta^s$ &c $+ .$ &c.

Mr. EULER gave the following reſolution, $x = \sqrt[n]{\pi} + \sqrt[n]{\varrho} + \sqrt[n]{\sigma} + \sqrt[n]{\tau} + \&c.$ where π, ϱ, σ, τ, &c. denote the roots of an equation of $n-1$ dimenſions $v^{n-1} - pv^{n-2} + qv^{n-3} - \&c. = 0$. It is evident, that in this caſe the equation whoſe root is x will have n^{n-1} dimenſions; for let the roots of the equation $z^n - 1 = 0$ be denoted by α, β, γ, δ, &c. then will the quantity $\sqrt[n]{\pi}$ have the n following values

lues $\alpha\sqrt[n]{\pi}$, $\beta\sqrt[n]{\pi}$, $\gamma\sqrt[n]{\pi}$, &c. and the same may be affirmed of the quantities $\sqrt[n]{\varrho}$, $\sqrt[n]{\sigma}$, $\sqrt[n]{\tau}$, &c. and consequently the quantity $\sqrt[n]{\pi}+\sqrt[n]{\varrho}$ will have $n\times n$ different values; and in the same manner the root $x=\sqrt[n]{\pi}+\sqrt[n]{\varrho}+\sqrt[n]{\sigma}+\sqrt[n]{\tau}+$ may be proved to contain $n\times n\times n\times n\times$ &c. $=n^{n-1}$ roots, and consequently in this resolution, in equations of superior dimensions, the number of independent coefficients $(n-1)$ will be very few in proportion to the number of dimensions n^{n-1}, or (if we respect its formula) n^{n-2} of the resulting equation.

Let $n=3$, and the equation resulting will rise to an equation of nine dimensions, which has the formula of a cubic; for let $x=\sqrt[3]{\pi}+\sqrt[3]{\varrho}=a$ one root, then will $\frac{-1+\sqrt{-3}}{2}a$ & $\frac{-1-\sqrt{-3}}{2}a$ be two other of the nine roots, and consequently the roots will be $\overline{x^3-a^3}\times\overline{x^3-b^3}\times\overline{x^3-c^3}$ $=0$, which has the formula of a cubic: and in general the above-mentioned equation of n^{n-1} dimensions will, for the same reason, have the formula of an equation of n^{n-2} dimensions.

Let the resolution be $x=\sqrt[2]{\pi}+\sqrt[2]{\varrho}+\sqrt[2]{\sigma}+\sqrt[2]{\tau}+$ &c. where π, ϱ, σ, τ, &c. denote the roots of an equation $x^{n-1}-px^{n-2}+qx^{n-3}-$ &c. $=0$ of $(n-1)$ dimensions, then will the resulting equation free from radicals, whose root is x, rise to 2^{n-1} dimensions; but as every affirmative

tive has a negative root equal to it, it will have the formula of an equation of 2^{n-2} dimenſions.

Let the reſolution be of this formula $x = \sqrt[r\times m]{\alpha} + \sqrt[r\times m]{\beta} + \sqrt[r\times m]{\gamma} + \sqrt[r\times m]{\delta} +$ &c. if α, β, γ, δ, &c. be conſidered as the r power of the roots of an equation of s dimenſions, then will the reſulting equation, of which the reſolution is given, riſe only to an equation of the formula of m^{s-1} dimenſions.

In the year 1762 I publiſhed ſome reaſons, for which this method could not extend to the general reſolution of algebraical equations.

XI. *Obſervations on the total (with Duration) and annular Eclipſe of the Sun, taken on the 24th of* June, 1778, *on Board the* Eſpagne, *being the Admiral's Ship of the Fleet of* New Spain, *in the Paſſage from the* Azores *towards* Cape St. Vincent's. *By Don* Antonio Ulloa, F. R. S. *Commander of the ſaid Squadron; communicated by* Samuel Horſley, *LL.D. F. R. S.*

Read December 24, 1778.

THE public prints will have given notice of my arrival at this port, with the fleet under my command, on the 29th of June laſt. A very favourable, though

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Obſervations de l'Eclipſe du Soleil totale avec Retenſion et Annulaire faites le 24^e^ Juin 1778; *ſur le Vaiſſeau* l'Eſpagne *Commandant l'Eſcadre de la Flotte de la* Nouvelle Eſpagne, *en faiſant le trajet des* Iſles Terceres *vers le* Cap St. Vincent. *Par* Don Antonio de Ulloa *chef d'Eſcadre et Commandant General de la ditte Flotte, Membre de la Societé Royale de* Londres.

ON aura appris par les nouvelles publiques, que je ſuis rentré dans ce port le 29^e^ du mois paſſé avec la flotte de la Nouvelle Eſpagne ſous mon commandement. Le trajet dans mon retour, qui a été long mais trés heureux, m'a été favo-

though long, paſſage gave me the opportunity of obſerving at ſea the eclipſe of the Sun, which was accompanied by a phenomenon obſerved by few aſtronomers, and which I myſelf ſaw for the firſt time; to wit, the luminous Annulus which ſurrounds the diſk of the Moon: the following is a deſcription of this appearance, one of the moſt beautiful that can be conceived.

The motion of the ſhip prevented our obſerving the beginning of the eclipſe, by reaſon of the difficulty there was in keeping the Solar image and a part of that of the Moon within the field of the teleſcope; the object vaniſhed every inſtant, and it was not till after ſeveral fruitleſs trials that I could get it again. Beſides this, the arms grew tired of holding up the teleſcope and ſmoaked glaſs, which could not be reſted upon any thing from the neceſſity there was of moving the teleſcope in a contrary

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rable pour obſerver en mer l'Eclipſe du ſoleil accompagnée d'un phenomene très particulier que j'ai vu, pour la prémiere fois et que peu d' aſtronomes, ont obſervé, juſqu' á préſent: c'eſt l'anneau lumineux autour du diſque de la lune, phenomène des plus frapans et des plus beau à la vue; en voici la deſcription:

Le mouvement du vaiſſeau ne permit pas d'obſerver le commencement de l'eclipſe, par la difficulté de conſerver le corps ſolaire et une partie de celui de la Lune dans le champ de la Lunette; l'objet m'échapoit à tout moment, et je ne le ratrapois qu'après bien des recherches. Outre cela, les bras ſe fatiguoient à ſoutenir en l'air la lunette et le verre obſcur, qu'on ne pouvoit apuyer, parce qu'il falloit

trary direction to that of the ſhip. I had no other calculations but thoſe to be met with in the *Connoiſſance des Tems*, which I did not find very exact, owing either to ſome error in the calculations themſelves, or to the longitude of the ſhip's place not having been accurately determined in that book: I found a pretty ſenſible difference in the hour ſet down for the beginning of the eclipſe. I obſerved the total obſcurity of the Sun's diſk at 44 minutes paſt three, the beginning of the emerſion at 48 minutes after three, the end of the eclipſe at 48 minutes after four; conſequently, the middle of it muſt have been at 46 minutes after three, or thereabouts: the total obſcurity laſted four minutes, a ſufficient time for obſerving the ring which was formed round the Moon.

Five

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falloit faire avec la Lunette un mouvement contraire à celui du vaiſſeau. Je n'avois d'autres calculs que celui de *la Connoiſſance des Tems* que je ne trouvois pas de la derniére préciſion, ſoit qu'il ſe trouve quelque erreur dans le calcul, ſoit que la longitude eſtimée du lieu ou le vaiſſeau ſetrouvoit ne fuſſe pas la veritable. Je trouvois une difference aſſez ſenſible ſur l'heure indiquée pour ſon commencement; J'obſervai la totale obſcurité du diſque du Soleil à 3 heures 44 min., le commencement de l'emerſion à 3 h. 48 m. la fin de l'eclipſe à 4 h. 48 m. par conſéquent le milieu devoit être à 3 h. 46 m. ou environ. La durée de l'eclipſe totale du Soleil fut de 4 minutes, intervale ſuffiſant pour obſerver l'anneau qui ſe forma autour de la Lune.

Five or ſix ſeconds after the immerſion we began to obſerve round the Moon a very brilliant circle of light, which ſeemed to have a rapid circular motion ſomething ſimilar to that of a rocket turning about its center. This light became livelier and more dazzling in proportion as the center of the Moon approached to that of the Sun, and about the middle of the eclipſe it was of the breadth of about a ſixth of the Moon's diameter. Out of this luminous circle there iſſued forth rays of light, which reached to the diſtance of a diameter of the Moon, ſometimes more, ſometimes leſs, which made me think that they were parts of a weaker light which were reflected in an atmoſphere more ſubtle than that in which the ring was formed. When the centers of the two planets began to ſeparate, the diminution began, and took

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5 ou 6 ſecondes après que l'immerſion eut été faite on commença à decouvrir autour de la Lune un cercle de lumiére très brillant qui ſembloit avoir un mouvement rapide circulaire, ſemblable a celui d'un artifice embraſé mis en jeu ſur ſon centre. Cette lumiére devint plus vive et plus éblouiſſante à meſure que le centre de la Lune aprochoit de celui du Soleil; et dans le temps que l'eclipſe fut à ſon milieu elle étoit large de deux doigs du diametre de la Lune, ou comme la ſixiéme partie du dit diamêtre. De ce cercle lumineux partoient des rayons de lumiére de toute ſa circomférence, perceptibles juſqu' à la diſtance d'un diamêtre de la Lune; tantôt plus, tantôt moins, ce qui me fit penſer que cétoit des parties de lumiére plus foibles qui s'imprimoient dans un atmoſphere plus ſubtil que celui ou étoit formé l'anneau. Lorſque les centres des deux planêtes commencérent à s'ecarter, la diminution commença et ſe fit graduellement dans le

même

took place gradually in the ſame order which had been obſerved at its beginning and during the progreſs of it. It diſappeared entirely four or five ſeconds before the emerſion. The colour of the light was not the ſame every where; the part immediately joining the diſk of the Moon was of a reddiſh caſt, from thence it changed towards a pale yellow, which about the middle began to clear till, at the external extremity, it ended in an almoſt entire white. It was equally brilliant throughout, and the whirling motion, common to all the parts of it, ſeemed to change the form and poſition of the rays which appeared to the eye ſometimes larger, ſometimes ſhorter, at the ſame time that there was no change either in the colours of the ring themſelves, or in the arrangement of them, both which continued as I have deſcribed them.

For

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même ordre qui ſ'étoit obſervé dans ſon commencement et ſes progrès : il diſparut entiérement 4 ou 5 ſecondes avant l'emerſion. La couleur de la lumiére n'etoit pas la même partout : la partie immediate au diſque de la Lune étoit couleur rougeatre, enſuite elle tiroit ſur le jaune pâle; et depuis le milieu juſqu'à l'extrêmité extérieure cette couleur jaune ſ'éclairciſſoit inſenſiblement juſqu'à tirer en fin tout a fait ſur le blanc. Elle étoit également brillante partout; et ſon mouvement de tourbillon, commun à toutes ſes parties paroiſſoit changer la forme et la poſition des rayons en les préſentant à la vue, tantôt plus courts, tantôt plus longs, ſans cependant occaſionner de changement dans les couleurs de l'anneau et leur arrangement qui reſtoit le même que je viens de détailler.

Quatre

For four or five ſeconds before the appearance of the ſhining ring, and during as many after it had diſappeared, one could ſee the ſtars of the firſt and ſecond magnitude as at the entrance of the night; but when it was in its greateſt degree of brilliancy, only thoſe of the firſt magnitude could be diſcovered. The darkneſs was ſuch, that perſons who were aſleep, and happened to wake, thought that they had ſlept the whole evening, and only waked when the night was pretty far advanced. The fowls, birds, and other animals on board took their uſual poſition for ſleeping as if it had been night.

Before the edge of the Sun's diſk emerged from that of the Moon, there was diſcovered near that of the latter a very ſmall point of that of the Sun; it was imperceptible to the naked eye, but having looked at it with

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Quatre ou cinq ſecondes avant de voir paroitre l'anneau brillant, et autant après ſa ſupreſſion l'on vit, comme à l'entrée de la nuit, les etoiles de la 1^ere^ et de la 2^de^ grandeur; mais lors qu'il étoit dans ſon brillant on ne voyoit que celles de la 1^ere^. L'obſcurité fut au point que des perſonnes qui dormoient et qui ſ'eveillérent crurent à leur grand étonnement d'avoir dormi toute la ſoirée et de ne ſ'être éveillées qu' aſſez avant dans la nuit. Les poules, les oiſeaux et les autres animaux prirent leur poſition ordinaire pour dormir comme ſi c'eut été la nuit.

Avant que le bord du diſque du Soleil parut par celui de la Lune, on decouvrit près de celleci un tres petit point de celui du Soleil imperceptible à la vue; mais l'ayant diſtingué par le ſecours de la lunette, je l'eſtimai d'abord de la grandeur

with the glaſs I eſtimated it at firſt to be about the magnitude of a ſtar of the fourth order; after which it ſeemed to increaſe to that of one of the third. Its firſt appearance, to wit, that before the edge of the Sun emerged from that of the Moon, laſted about a minute and a quarter, the luminous circle was ſtill viſible though already much weaker than it had been.

The reddiſh colour of the ring towards the Lunar diſk, its deep yellow towards the middle, its clear and very pale yellow at the external extremity, its uniform circumference, and the rays iſſuing from it to the diſtance taken notice of above, convince me that the whole is the effect of the Lunar atmoſphere, which is of a ſubſtance different from that of the earth, that is, more tranſparent, more homogeneous, more uniform, and fitter

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grandeur d'une etoile de la 4^{e} claſſe; et enſuite il me parut augmenter juſqu'à la grandeur de celles de la 3^{e}. Sa prémiére apparition, c'eſt à dire avant que le bord du Soleil parut par celui de la Lune, fut de la durée d'environ une minute et un quart: le cercle lumineux ſubſiſtoit encore quoique deja affoibli.

La couleur rougeatre de l'anneau proche du diſque de la Lune, jaunatre comme couleur d'or vers le milieu, jaune clair et tres affoibli vers ſa partie extérieure; ſa circonferénce egale, et les rayons qui partent de cet anneau à la diſtance ditte ci deſſus perſuadent que le tout eſt l'effet de l'atmoſphere de la Lune, laquelle eſt de matiére differente de celle de la terre; plus tranſparente, plus nette, plus egale et plus propre à réfléchir les rayons de lumiére que celle ci; autrement, l'anneau n'auroit pas été également clair, brillant et coloré dans la circonference entiére du diſque de la lune. On ne peut pas dire que cet anneau lumineux ſoit

ter for reflecting the rays of light, ſince otherwiſe the ring would not have been equally clear, ſhining, and coloured throughout the whole circumference of the Lunar diſk. It cannot be ſaid, that this luminous ring is the effect of the rays of the Sun reflected by the atmoſphere of the earth, becauſe the apparent diameter of the Sun is ſmaller than that of the Moon, whoſe diſk entirely hid that of the Sun. Beſides, if the luminous circle had been made by the atmoſphere of the earth, its colours would have been like thoſe of the rainbow, and it would have appeared fixed without motion, inſtead of which, that which was ſeen is the ſame as that which is ſeen by the naked eye upon the Sun when it is juſt above the horizon a little after Sun-riſe or before Sun-ſet, ſo that one may conclude, that this luminous circle is a part of the diſk of the Sun ſeen after refraction through the atmoſphere of the Moon.

The

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l'effet des rayons du Soleil reflechis ſur l'atmoſphere de la Terre, puiſque le diamêtre apparent du Soleil eſt plus petit que celui de la Lune, dont le diſque cachoit entiérement à nos regard ou à ceux du globe terreſtre celui du Soleil. D'ailleurs, ſi cétoit ſur l'atmoſphere de la Terre que le cercle lumineux fut formé, ce cercle auroit été ſemblable dans ſes couleurs à l'arc-en-ciel, et il auroit paru fixe ſans mouvement ; au lieu que celui qui a été aperçu eſt le même qu'on diſtingue dans le Soleil en le regardant directement avec la ſimple vue ſur l'horizon peu après ſon lever ou un peu avant ſon coucher : enſorte qu'on peut conclure que ce cercle lumineux eſt une partie du diſque du Soleil vu par refraction ſur l'atmoſphere de la Lune.

The point of the Sun's diſk, which was ſeen before its limb began to emerge from that of the Moon, is a very extraordinary phenomenon which I was not acquainted with before. In order to obviate all doubts which might ariſe about it, I muſt mention that we were three obſervers, Don JOACHIN D'ARANDA, Lieutenant WINTUYSEN, and I. Mr. D'ARANDA, who was looking at the eclipſe through a two-foot teleſcope about the end of the total obſcurity, was the firſt who perceived it. He, not knowing what it was, told me, that the total obſcurity drew near an end, becauſe he diſcovered a ſmall point of the Sun, like a ſtar, on the edge of the Moon. I looked immediately with the naked eye, and ſaw nothing. I then took out a ſpy glaſs, with which I ſaw as much. At length I took out my teleſcope of two feet and a half, and did diſcover with that a red luminous point ſo near the

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Le point du diſque du Soleil, vu avant que ſon limbe euſſe commencé à paroitre par celui de la Lune, eſt un phenomène tres particulier et dont je n'avois pas connoiſſance. Pour prevenir les doutes qui peuvent ſe lever je dois dire que nous étions trois perſonnes à obſerver l'eclipſe: M. M. Don JOACHIN DE ARANDA Cap. de frégate de l'armée, et pilotte major de la flotte; Don P. WINTUISEN lieutenant de vaiſſeau et major de la ditte flotte, et moi. Mr. D'ARANDA, en regardant l'eclipſe vers la fin de la totale obſcurité avec ſa lunette de deux pieds, fut celui qui l'aperçut le premier: Il me dit, ne ſachant ce que c'étoit, que la totale obſcurité étoit près de finir, parce qu'il voyoit un petit point du Soleil ſur le bord de la Lune ſemblable à une etoile. Je regardai d'abord avec la vue,

the edge of the Moon, that it left me no doubts of its belonging to the body of the Sun. I, at that time, eſtimated it to be about the ſize of a ſtar of the third magnitude; and imagine, that when Mr. D'ARANDA diſcovered it, it muſt have been like one of the fourth. This point gradually increaſed, and when it became of the bigneſs of a ſtar of the ſecond magnitude, the edge of the Sun emerged from that of the Moon. The interval between the firſt diſcovery of this point and the beginning of the emerſion was about a minute and a quarter. This apparition of the Sun, before the beginning of the emerſion, can only have taken place through ſome crevice or inequality on the limb of the Moon, not perceivable at the full Moon, by reaſon of the reflected rays which croſs each other, and confuſe it; whereas at the time of the eclipſe,

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et je n' aperçu rien : Je tirai une lorgnette de la poche et je n'en vis pas d'avantage; enfin je pris ma Lunette de $2\frac{1}{2}$ pieds avec laquelle je découvris effectivement un point rouge et lumineux tres proche du bord de la Lune qui ne me laiſſa aucun doute que ce ne fut le corps du Soleil. Je l'eſtimai pour lors comme une etoile de la 3^{e} grandeur, et je préſume que lors que M. D' ARANDA la découvrit, elle pouvoit paroitre comme une étoile de la 4^{e} grandeur. Ce point grandit ſucceſſivement; et lorqu'il pouvoit être jugé comme une etoile de la 2^{e} grandeur le bord du Soleil parût par celui de la Lune. L'intervale qu'il y eut entre la 1^{ere} viſion de ce point et le commencement de l'emerſion fut d'une minute et un quart. Cette apparition du Soleil avant le commencement de l'emerſion ne peut avoir eu lieu qu'a travers de quelque fente ou inégalité qui ſe trouve ſur le limbe de la Lune, et qui eſt imperceptible lors du plenilune à cauſe des rayons reflechis qui ſe croiſent et confondent la ditte ouverture; au lieu que dans le

eclipſe, the Moon's body being entirely obſcured, the light of the Sun is behind, and comes through the ſmalleſt openings in the diſk without any confuſion.

The time elapſed between the firſt appearance of the Sun's body through the aperture of the limb of the Moon and the appearance of the limb of the Sun out of that of the Moon will ſerve to determine the depth of the ſaid chink, aperture, or inequality, which is equal to the height of the eminencies which form it.

The luminous point was towards the North-weſt part of the Moon's diſk, a little more to the North than the part of its limb through which that of the Sun appeared at the beginning of the emerſion; and it is remarkable, that no other luminous ſpeck was perceived in the diſk beſides this. This aperture is therefore the only one in that part of the diſk through which the emerſion was to

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tems de l'eclipſe le corps de la Lune ſe trouvant entiérement obſcurci, la lumiére du Soleil ſe trouve par derriére et perce ſans confuſion par les plus petites ouvertures du diſque de cet aſtre.

Le tems qui ſ'eſt écoulé depuis la premiére apparition du corps du Soleil par l'ouverture du limbe de la Lune juſqu'à l'apparition du limbe du Soleil par celui de la Lune ſervira à determiner la profondeur de la ditte fente, ouverture, ou inégalité qui eſt égale à la hauteur des éminences qui la forment.

Le dit point lumineux étoit à la partie du Nord-Oueſt du diſque de la Lune, un peu plus au nord de l'endroit de ſon limbe par où ſe fit voir celui du Soleil au commencement de l'emerſion; et il eſt à remarquer qu'on n'aperçut pas d'autre, point lumineux dans le diſque de la Lune que celui là: ainſi cette ouverture eſt unique

to begin; from whence one may be certain, that throughout the fourth part of the Moon's circumference, reaching from North to Weſt, there is not any perceptible breaks in its limb beſides that which was then obſerved. There can be no doubt but that the luminous ſpeck which appeared through the aperture was part of the Sun's body; this is demonſtrated, by the red fiery colour (the ſame as that which is ſeen when this luminary is looked at through a ſmoaked glaſs) by its gradual increaſe, in proportion as the limb of the Sun came near that of the Moon, and in ſhort by the colour, which at its emerging was juſt the ſame as that which had been ſeen through the opening.

It remains to be mentioned, that on the 24th day of July, the day on which I made the above obſervation

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unique dans la partie du diſque par ou devoit commencer l'emerſion, et on peut aſſurer que dans la quatrieme partie de la circonference de la Lune, il n'y a pas dans ſon Limbe, depuis le Nord juſqu'à l'oueſt d'autres fentes perceptibles que celle qui fut obſervée. Il n'y a point de doute que le point lumineux qui parut à travers l'ouverture ne fut le corps du Soleil; Cela eſt démontré par la couleur de feu trés rouge, la même qui ſe voit quand on regarde cet Aſtre à travers d'un verre obſcurci, par la gradation qui eut lieu dans ſon accroiſſement à meſure que le Limbe du Soleil aprochoit de celui de la Lune, et enfin, par la couleur du Soleil, qui lors qu'il deborda fut la même que celle qui s'étoit vue à travers de l'ouverture.

Il me reſte à dire que le 24^{e} Juin, jour que je fis cette obſervation à bord du Vaiſſeau

vation on board the Eſpagne, the aforeſaid ſhip's latitude was 37° 14″ North, meaſured the ſame day; that ſince noon it had ſailed direct Eaſt; that from the end of the eclipſe to its being in the meridian of Cape St. Vincent it had ſailed 301 ſea miles Eaſt, making 100 ſea leagues, reckoning 20 leagues to a degree, remains to be known the difference of meridians of the ſaid Cape and the meridian of the different obſervatories of the capital cities of Europe, in order to determine the part of the ſea in which the obſervation was made relatively to the obſervatories.

The ſolar ſpots were ſeen very diſtinctly both before and after the eclipſe; there were ſix of them in all: two very near each other on the Eaſtern part of the diſk; two towards the middle of the diſk, likewiſe very near one another;

Vaiſſeau l'Eſpagne commandant l'eſcadre de la flotte de Vera Cruz, ce V^au étoit par 37 degrés 14 minutes de Latitude Septentrionale, obſervée le même jour; que depuis le Midy il avoit fait route a l'eſt directement; que depuis la fin de l'Eclipſe juſqu'a être nord ſud avec le Cap St. Vincent il a navigué à l'eſt 301 milles maritimes qui font 100 $\frac{1}{3}$ lieues maritimes de 20 au degré; reſte à ſavoir la difference de Méridien entre le dit Cap et les different Obſervatoires des villes capitales de l'Europe, pour determiner l'endroit à la mer ou l'obſervation fut faite relativement aux dits Obſervatoires.

Les taches du Soleil furent vues tres diſtinctement avant et après l'Eclipſe; elles étoient au nombre de ſix: deux à la partie de l'eſt du diſque peu diſtantes l'une de l' autre: deux vers le milieu du diſque auſſi aſſez près l'une de l'autre: et

another; and two towards the North, verging towards the North-weſt.

The corrected altitude of the center of the Sun above the horizon, taken at the moment the eclipſe ended, was 36° 31″. The atmoſphere was very clear; the wind WNW, and not very ſtrong. No clouds were viſible, as is often the caſe at ~~the~~ ſea, nor were there any till near ſix, when there aroſe ſome juſt above the horizon.

The luminous ring made ſo ſtrong an impreſſion on me, as well from the beauty and brightneſs of its colour as from its rapid and uniform circular motion that I could neither reckon the ſtars viſible in each interval from the total obſcurity to the end of the eclipſe, nor make ſeveral other obſervations on the colour and vivacity of their light.

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et deux enfin vers la partie du Nord un peu vers Nordoueſt.

La hauteur corrigée du centre du Soleil ſur l'horizon, priſe au moment que l'Eclipſe finit étoit 36 degres 31 m. L'atmoſphere étoit très net et l'air de l'Oueſt Nord-Oueſt de moyenne force; on ne voyoit aucun nuage comme cela arrive ſouvent en mer; ce ne fut que vers les ſix heures qu'il ſ'en forma quelques uns ſur l'horizon.

L'anneau lumineux me fit une impreſſion ſi agreable tant par la beauté et l'eclat de ſa couleur que par ſon mouvement circulaire uniforme et rapide que je ne pus ni compter les etoiles viſibles dans chaque intervale depuis la totale obcurité juſqu'á la fin, ni faire d'autres obſervations ſur la couleur et la vivacité de leur lumiére: Je m'attachai uniquement à l'anneau et enſuite au point lumineux

light. All I attended to was, firſt, the ring, and then the luminous ſpeck which appeared through the diſk of the Sun. Theſe made the ſame impreſſion on all who obſerved with me.

It is not eaſy to make celeſtial obſervations at ſea with the ſame preciſion and delicacy as they are made at land, on account of the motion of the ſhip, and want of convenience to uſe the inſtruments. It would have been difficult (even if we had had a Micrometer) to have meaſured either the breadth of the ring, in order to ſee if it was equal every where, or the diſtance between the luminous ſpeck ſeen on the diſk of the Moon and its limb. I regret, however, exceedingly not having been able to make obſervations which would have been of ſuch uſe in aſtronomy.

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neux du Soleil au travers du diſque de la Lune. Ce raviſſement produiſit le même effet ſur ceux qui obſervoient avec moi.

Il n'eſt pas facile de faire en mer des Obſervations Celeſtes avec autant de préciſion et de delicateſſe qu'on les fait à terre, à cauſe du mouvement du vaiſſeau et de la gène à ſe ſervir des inſtrumens. Il auroit été difficile, quand meme on auroit été pourvu d'un Micromêtre, de meſurer la largeur de l'anneau pour examiner ſ'il étoit égal partout, comme auſſi la diſtance du point lumineux vu ſur le diſque de la Lune juſqu'a ſon limbe. J'ai bien du regret de n'avoir pu faire ces obſervations qui auroient été d'un grand avantage pour la Phiſique des aſtres.

XII. *Tentamen continens Theoriam Machinæ ſublicarum.* *Autore* Thoma Bugge, *Aſtronomo Regio, Aſtron. et Mathem. Prof. in Academia* Havnienſi, *e Societatibus Scient.* Havnienſ. *et* Nidroſ. *Communicated by Sir* John Pringle, *Bart. F. R. S.*

Read Dec. 24, 1779.

INTER innumera commoda, quæ ſocietati civili adfert Mechanica, haud minimum eſt ars ſublicas adigendi, ſeu, palos majores trabeſque oblongas terræ impingendi. Artem hanc veteribus non fuiſſe ignotam ex pluribus VITRUVII locis poteſt probari. Etiam ſi celeberrimus hic antiquitatis autor machinam non deſcribat, tamen extra omne dubium poſita eſt veterum in hac arte peritia. Sine ea enim impoſſible fuiſſet exſtruere pontes, molas, aggeres, pyramides, columnas, ædificia, quorum molem, majeſtatem, firmitatemque venerabundi admiramur et vix imitari audemus. Hæc omnia requirunt fortiſſimas et ſolidiſſimas ſubſtructiones. Si loca ſint congeſtitia et paluſtria, ſublicæ machinarum vi adiguntur, tunc imponitur craticula; plures ſublicæ impinguntur, capitibus promi-

prominentibus; earum intercapedines opplentur lapidibus majoribus, ſilicibus, arena majori foſſitia et mortario, quibus fundamentis demum ſuperſtruenda ſunt ædificia.

Forma machinæ, qua veteres architectones ſublicas adegerunt, non ſatis conſtat. Recentiores varias ei dederunt compages et configurationes. Complures deſcripſerunt LEOPOLD, DESAGULIERS, et BELIDOR. Inter has eminet et palmam omnibus præripit ſublicarum machina VAULOUE inventa, a DESAGULIERS deſcripta, ac in uſum perducta dum fundamenta pontis Weſtmonaſterienſis conjicerentur. Præcipua ejus commoda ſunt, ut ad onus (quod arietem vocare licet) elevandum minor requiratur hominum numerus, ut aries ad majorem elevatus altitudinem libere decidat, utque arietem deciduum luſu machinæ iterum arripiat forceps, et mox elevet; quibus machinamentis breviſſimo temporis ſpatio et pauciſſimis operariis maximus ſublicarum numerus ad maximam profunditatem adigi poſſunt.

Theoriam effectus hujus machinæ quidem dedit BELIDOR. Sed, quantum video, eam ſuperſtruxit fundamento prorſus erroneo et parum ſolido; eam deducit ex regulis colliſionis corporum, conſiderando ſublicam terræ impingendam et onus deciduum tanquam corpora colliſa. Quis autem non vidit regulas colliſionis ſupponere motum liberum, et medium non reſiſtens? Nullo ergo jure

in dato caſu, ubi ſublicæ ſoli reſiſtentia valde magna opponitur, applicari poteſt. Tentabimus aliam explicare hujus machinæ theoriam.

Res eo redit, ut onus ingens ex certa altitudine cadens percutiat ſublicarum capita annulo ferreo cincta; conſiderabimus duas machinas; vocabimus onera cadentia = o et *o*; altitudines, e quibus decidunt = A et *a*; ſublicarum adigendarum maſſas = M et *m*; ſuperficies earum, quoad terræ impactæ ſunt = s et *s*; et profunditates ſublicarum in ſolo = P et *p*. Percuſſio oneris cadentis inſtar virium vivarum eſt æſtimanda per productum ex maſſa oneris in quadratum celeritatis; hoc autem quadratum proportionale eſt altitudini, ex qua cadit onus; proinde percuſſio æſtimari poteſt per factum ex maſſa oneris impingentis in altitudinem computatam a ſupremo puncto ad caput ſublicæ. Sed effectus ſunt uti vires cauſarum ſuarum plenæ. Ergo, ſi reſiſtentiam ſoli et maſſas ſublicarum utrobique æquales ſtatuamus, profunditates, ad quas ſingulis percuſſionibus adiguntur ſublicæ, erunt in ratione compoſita e rationibus directis onerum et altitudinum; ſeu

$$p : P = a \times o : A \times O.$$

Si ſumamus cohærentiam ſoli æqualem et homogeneam, reſiſtentia, quam, dum ſubſident, vincere debent ſublicæ, creſcit in ratione ſuperficierum ſolo impactarum. Si

jam

jam ſtatuamus onera cadentia æqualia o=*o*; altitudines quoque æquales A=*a*; patet effectus percuſſionum et hinc profunditates decreſcere prout creſcunt tam ſuperficies adactæ quam ſublicarum pondera vel maſſæ. Hinc ſub data hypotheſi, erunt profunditates in ratione compoſita e rationibus inverſis ſuperficierum et maſſarum, ſeu

$$p : \mathrm{P} = \mathrm{S} \times \mathrm{M} : \int \times m = \frac{\mathrm{I}}{\int \times m} : \frac{\mathrm{I}}{\mathrm{S} \times \mathrm{M}}.$$

Si jam omnia ſunt inæqualia, nempe onera cadentia, altitudines, maſſæ ſublicarum, et earum ſuperficies in terra conditæ; dico profunditates, ſingulo ictu acquiſitas, eſſe in ratione compoſita e rationibus directis onerum cadentium et altitudinum, et e rationibus inverſis ſuperficierum et maſſarum. Concipiamus tertiam ſublicam, cujus maſſa = M, ſuperficies adacta = S; onus impingens = O; altitudo, e qua cadit onus = *a*; dicatur profunditas hujus ſublicæ ex data percurſione proveniens = π. Tunc erit per antea demonſtrata

$$p : \pi = \frac{\mathrm{I}}{m \times \int} : \frac{\mathrm{I}}{\mathrm{M} \times \mathrm{S}}.$$

$$\pi : \mathrm{P} = a \times o : \mathrm{A} \times \mathrm{O}.$$

$$\text{Ergo } p : \mathrm{P} = \frac{a \times o}{m \times \int} : \frac{\mathrm{A} \times \mathrm{O}}{m \times \mathrm{S}}.$$

Hæc theoremata inſerviunt diverſis ſublicarum machinis comparandis, praxique exercendæ. Ad determinandum

ſumtus operis exſtruendi maxima ſunt utilia; iiſque ſuperſtruimus ſequens problema.

Calculo definire profunditatem ad quam ſingulo ictu ſubſidet ſublica datæ machinæ vi adacta.

Cum in hoc caſu tam onus cadens quam maſſa pali impingendi conſtans ſit et æqualis; erit $o = o$, et $M = m$. Hinc explicata proportio fundamentalis in ſequentem abit.

$$p : P = \frac{a}{s} : \frac{A}{S}.$$

Porro ſuperficies ſublicarum terræ impactarum ſunt rectangula eandem baſin ſed diverſam altitudinem p et P habentia (P hic ſignificat profunditatem totalem). Quapropter $s : S = p : P'$, id quod ſimpliciorem et commodiorem ſubminiſtrat analogiam:

$$p : P = \frac{a}{p} : \frac{A}{P'}.$$

Poſt percuſſiones quaſdam factas, adeo ut firmiter terræ inhæreat ſublica, fiat denique novus ictus, quem pro primo numeramus, tum cadat onus ex altitudine $= a$ et ſubſideat ſublica profunditate $= p$. Fiat tum ſecundus ictus, tunc ſubſidebit palus profunditate $= x$. Onus cadet per altitudinem $A = a + p$, et ſublicæ profunditas totalis erit $= P = p + x$. Facta jam debita ſubſtitutione habemus.

$$p : x = \frac{a}{p} : \frac{a+p}{p+x}.$$

Ex

Ex qua analogia originem ducit ſequens æquatio.

$$px+x^2=p^2+\frac{p^3}{a}.$$

Quæ æquatio quadratica, ſi reſolvatur, dabit valorem incognitæ

$$x=\pm\sqrt{\frac{5}{4}p^2+\frac{p^3}{a}}-\frac{1}{2}p.$$

Applicemus calculum ad datum exemplum. Sit altitudo, e qua decidit onus percutiens $a=3$ ped $=36$ pol. Profunditas ad quam primo ictu ſubſidet ſublica $=p=4$ pol. dicatur jam profunditas ad quam ſecundo ictu ſubſidebit $=x$; erit:

$$\frac{4:x=\frac{36}{4}:\frac{40}{4+x}}{\frac{4x+x^2=\frac{160}{9}}{x=2\frac{2}{3}\text{ pol.}}}$$

Secundo ictu ſublica ſubſidet numero rotundo 3 pol. In tertio ictu erit altitudo, quam percurrit onus impingens $=36+4+3=43$ pol. Profunditas ſublicæ $=4+3=7$ pol. Denique profunditas tertio ictu acquiſita $=x$; tunc,

$$\frac{4:x=\frac{36}{4}:\frac{43}{7+x}}{\frac{7x+x^2=\frac{172}{9}}{x=\frac{11}{7}:\frac{7}{2}=2\text{ pol. quam proxime.}}}$$

In quarto ictu eſt altitudo $=36+4+3+2=45$ pol. et profunditas ſublicæ $=4+3+2=9$ pol. Profunditas quarto ictu acquiſita $=x$; tunc erit,

4 : *x*

$$\frac{4 : x = \frac{36}{4} : \frac{45}{9+x}}{9x+x^2 = \frac{180}{9}}$$

$x = \frac{12}{2} - \frac{9}{2} = 1\frac{1}{2}$ pol. quam proxime.

Idem problema ſibi ſolvendum ſumſit D. BELIDOR. Supponit ſublicam primo ictu ſubſidere 15 pol. Tunc juxta ejus calculos ſecundo ictu ſubſidebit 17; tertio 19; quarto 21; quinto 23. Quis autem non vidit hanc progreſſionem cum theoria et experientia pugnare? Profunditas enim ſingulo ictu acquiſita continuo decreſcit, et tandem ſublica repetitis ictibus non amplius ſubſidet. Id quod evenit, quando cohærentia ſoli et frictio major eſt vi a percuſſione oriunda. Eo in caſu ſublica profundius adigi non poteſt niſi augeantur vel onus percutiens vel altitudo, e qua cadit.

In actis Academiæ Stockholmienſis mechanicus quidam haud incelebris ſtatuit ſublicam ponderibus oneratam in hoc caſu ulterius et profundius in terram adigi poſſe. Verum ſi ſublica ita oneratur, idem eſt ac ſi ſublicæ maſſa major eſſet, quod non auget ſed impedit effectum percuſſionis. Ad adigendam ſublicam pondus impoſitum agit ſola preſſione, quæ pro inſenſibili eſt habenda reſpectu reſiſtentiæ et frictionis. Si vero onus impingens augeatur, certiſſime quoque augebitur percuſſio. Quam rem theoria et experientia confirmant; notum enim

enim eſt ex experimentis D. CAMUS malleum 12 librarum ambabus manibus elevatum et proinde ex altitudine 5 pedum cadentem eundem effectum reddere ac ſimplex preſſio 1000 librarum.

Determinare maximam profunditatem, ad quam ſublica data machina datâ adigi poteſt.

Sit altitudo, quam onus percurrit in primo ictu $= a$; profunditas per primum ictum acquiſita $= p$; poſt factas complures percuſſiones ſublica ſubſidet quantitate admodum parva $= m$; et tum operæ pretium non eſt plures dare percuſſiones. Profunditas totalis antea acquiſita $= x$; et altitudo, quam onus percutiens deſcribit $= a+x$. Hinc,

$$p : m = \frac{a}{p} : \frac{a+x}{x}.$$

Ex reſolutione hujus æquationis invenitur,

$$x = \frac{ap^2}{am-p^2}.$$

Sit $p=4$ pol.; $m=\frac{1}{10}$ pol.; $a=36$ pol.; invenitur maxima profunditas $x = \frac{36\times 16}{\frac{16}{10}-16} = -576 : \frac{124}{10} = -46{,}6$ pol. Hæc quantitas negativa eſſe debet, cum oppoſita ſit altitudini, quæ inſtar poſitivæ eſt aſſumpta.

Corodinis loco ſequentes practicas obſervationes adjungimus. 1. Pondus oneris impingentis ut plurimum eſt 800 libr. Vis hominis onus continuo labore ele-

$$\frac{4 : x = \frac{36}{4} : \frac{45}{9+x}}{9x+x^2 = \frac{180}{9}}$$

$$x = \frac{12}{2} - \frac{9}{2} = 1\frac{1}{2} \text{ pol. quam proxime.}$$

Idem problema ſibi ſolvendum ſumſit D. BELIDOR. Supponit ſublicam primo ictu ſubſidere 15 pol. Tunc juxta ejus calculos ſecundo ictu ſubſidebit 17; tertio 19; quarto 21; quinto 23. Quis autem non vidit hanc progreſſionem cum theoria et experientia pugnare? Profunditas enim ſingulo ictu acquiſita continuo decreſcit, et tandem ſublica repetitis ictibus non amplius ſubſidet. Id quod evenit, quando cohærentia ſoli et frictio major eſt vi a percuſſione oriunda. Eo in caſu ſublica profundius adigi non poteſt niſi augeantur vel onus percutiens vel altitudo, e qua cadit.

In actis Academiæ Stockholmienſis mechanicus quidam haud incelebris ſtatuit ſublicam ponderibus oneratam in hoc caſu ulterius et profundius in terram adigi poſſe. Verum ſi ſublica ita oneratur, idem eſt ac ſi ſublicæ maſſa major eſſet, quod non auget ſed impedit effectum percuſſionis. Ad adigendam ſublicam pondus impoſitum agit ſola preſſione, quæ pro inſenſibili eſt habenda reſpectu reſiſtentiæ et frictionis. Si vero onus impingens augeatur, certiſſime quoque augebitur percuſſio. Quam rem theoria et experientia confirmant; notum enim

chim eſt ex experimentis D. CAMUS malleum 12 librarum ambabus manibus elevatum et proinde ex altitudine 5 pedum cadentem eundem effectum reddere ac ſimplex preſſio 1000 librarum.

Determinare maximam profunditatem, ad quam ſublica data machina datâ adigi poteſt.

Sit altitudo, quam onus percurrit in primo ictu $= a$; profunditas per primum ictum acquiſita $= p$; poſt factas complures percuſſiones ſublica ſubſidet quantitate admodum parva $= m$; et tum operæ pretium non eſt plures dare percuſſiones. Profunditas totalis antea acquiſita $= x$; et altitudo, quam onus percutiens deſcribit $= a + x$. Hinc,

$$p : m = \frac{a}{p} : \frac{a+x}{x}.$$

Ex reſolutione hujus æquationis invenitur,

$$x = \frac{ap^2}{am - p^2}.$$

Sit $p = 4$ pol.; $m = \frac{1}{10}$ pol.; $a = 36$ pol.; invenitur maxima profunditas $x = \frac{36 \times 16}{\frac{16}{10} - 16} = -576 : \frac{124}{10} = -46{,}6$ pol. Hæc quantitas negativa eſſe debet, cum oppoſita ſit altitudini, quæ inſtar poſitivæ eſt aſſumpta.

Corodinis loco ſequentes practicas obſervationes adjungimus. 1. Pondus oneris impingentis ut plurimum eſt 800 libr. Vis hominis onus continuo labore ele-

vantis circiter 40 lib. æſtimanda eſt. Hinc in machina ſimplici 20 homines onus elevare valent. Ictibus autem 25 vel 30 datis, ut per æquale temporis ſpatium requieſcant neceſſe eſt. 2. Ad percuſſiones 30 dandas et requiem capiendam 4 minuta prima requiruntur, adeo ut per integram horam 450 ictibus ſublicam percutere liceat. 3. Virium impedimentum eſt, ſi diameter trachleæ ſuperioris, cui funis onus elevans circumvolvitur ſit juſto minor. 4. Si onus impingens ope manuum et non mediante axi in peritrochio eſt elevandum, altitudo, quam percurrit onus, non ſuperare poteſt 5 pedes quos ſolummodo emetiri poſſunt homines brachiorum liberô motu. 5. Quando plures adigendæ ſunt ſublicæ, prodeſt opus incipere a medio, et ad extremitates procedere, ſi enim contrario modo rem aggreſſus fueris, ſublicæ intermediæ adigi non poſſunt ſine opere et temporis diſpendio propter ſoli compactionem. 6. Antequam opus incipiatur, terra eſt examinanda et ſublica probatoria adigenda, ut exactam de ſoli qualitate ejuſque ſtratis habeas cognitionem. 7. His præſuppoſitis, æſtimari poſſunt ſumtus ad ſeriem ſublicarum adigendam neceſſarii. Ad machinam transferendam et ſingulas ſublicas perpendiculariter erigendas requiruntur 15 minuta prima. Ex ictibus probatoriis jnxta precedentia

problemata calculari poſſunt numerus percuſſionum per ſingula ſtrata et hinc tempus totum requiſitum. Tandem ex longitudine operis, ex pretio et numero ſublicarum æſtimari poſſunt ſumtus omnes quos impendere oportet.

Havniæ, 30 Auguſti 1778.

M. JEAURAT, de l'Académie Royale de Paris, ayant inventé une nouvelle machine catoptrique, appellée Télescope, thought proper to communicate to the Royal Society a short description of this new invention.

This Telescope, or Catoptric Heliometer, because it produces two images of the objects, the one in a direct position, and the other reversed. These two images

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XIII. *Account of an Iconantidiptic Teleſcope, invented by Mr.* Jeaurat, *of the Academy of Sciences of Paris. Communicated by* John Hyacinth de Magellans, *F. R. S.*

Read January 19, 1778.

MR. JEAURAT, of the Royal Academy of Sciences of Paris, having diſcovered a conſtruction of the Iconantidiptic Teleſcope, thought proper to communicate to the Royal Society of London a ſhort deſcription of this new invention.

This Teleſcope is called the Iconantidiptic Heliometer, becauſe it produces two images of the objects, the one in a direct poſition, and the other reverſed. Theſe two images,

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Conſtruction d'une lunette Iconantidyptique inventée par Mr. Jeaurat, *de l'Academie Royale des Sciences de* Paris.

M. JEAURAT, de l'Academie Royale de Paris, ayant imaginé une conſtruction de lunette Iconantidyptique, croit devoir communiquer à la Societé Royale de Londres l'expoſition ſuccinte de cette nouvelle invention.

La lunette eſt appellée Heliometre Iconantidyptique parce qu'elle repreſente deux images des objets, l'une dans une ſituation droite et l'autre dans une ſitua-

tion

images, of oppoſite ſituation to each other, are exactly of the ſame ſize, and produce the effect of ſhewing the ſtars as entering at once both on the right and left ſides of the Teleſcope. The firſt coincidence of the two images on the ſide of each other gives the paſſage of the firſt limb; the exact coincidence of the two images upon one another gives the paſſage of the center of the ſtar; and the laſt coincidence of the two images at the ſide of each other gives the paſſage of the ſecond edge: from whence it follows, that we not only obſerve as uſual the paſſage of the two ſides of the diſk of a ſtar, but alſo the direct paſſage of the center of the ſtar: an obſervation which could not before be made in a direct manner. Beſides, it may be obſerved, that this invention obviates the difficulty of illuminating the threads of the Teleſcope in obſerving very

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tion renverſée. Ces deux images de ſituation oppoſée l'une à l'autre ſont exactement de la même graudeur et produiſent l'effet de voir tout à la fois entrer les aſtres par la droite et par la gauche de la lunette. La premiere coincidence des deux images à coté l'une de l'autre donne le paſſage du premier bord, la coincidence exacte des deux images l'une ſur l'autre donne le paſſage du centre de l'aſtre, et la derniere coincidence des deux images l'une à coté de l'autre donne le paſſage du ſecond bord, d'où il ſuit que non ſeulement on obſerve comme à l'ordinaire le paſſage des deux bord du diſque d'un aſtre, mais auſſi le paſſage direct du centre de l'aſtre: obſervation qui n'a juſq'à preſent pû ſe faire d'une maniere directe. D'ailleurs on remarquera que cette invention ſupplée a la difficulté d'éclairer les fils de la lunette lorſqu'il eſt queſtion d'obſerver de très petites etoiles, car avec cette

very ſmall ſtars, for in this conſtruction there is no occaſion to ſee the threads.

The following is the conſtruction of this Iconantidiptic Teleſcope, which I have already made uſe of, and which appears to be proper for obſervations made in the plane of the Meridian.

That the ſolution may be applicable to Teleſcopes, it is proper that AD=AZ, $aB=aZ$.

Then put AD=F the focal diſtance of the lens A,

$aB=f$ the focal diſtance of the lens a,

$aA=F-f$,

$AB=aA-aB=F-2f$,

BC

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conſtruction on n'a pas beſoin de voir les fils.

Voici la conſtruction de cette lunette Iconantidyptique dont je me ſuis deja ſervie et qui me paroit commode pour les obſervationsfaites dans le plan du Meridien.

Pour que la ſolution ſoit auſſi appliquable aux oculaires il convient que $AD=AZ$, $aB=aZ$.

Alors on fera AD=F foyer de la lentille A,

$aB=f$ foyer de la lentille a,

$aA=F-f$,

$AB=aA-aB=F-2f$,

BC

BC $= x$,

CD $= y$,

φ the focal distance of the lens C.

Hence $\begin{cases} BD = AD + AB = 2 \times \overline{F - f}, \\ BD = BC + CD = x + y. \end{cases}$

The two values of BD evidently give, 1st, $x + y = 2 \times \overline{F - f}$.

That the image B, given by the lens a, be seen at the distance BC: and that the direction of the ray BHD may form a relative focus in D, whose distance may be equivalent to AD, it is necessary that $aB \times CD = AD \times BC$, namely, 2dly, $fy = Fx$.

That the object B, seen in the direction BH, may form a focus in D, it is necessary that the focal distance of the lens

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BC $= x$,

CD $= y$,

φ foyer de la lentille C.

$\begin{cases} BD = AD + AB = 2(F - f), \\ BD = BC + CD = x + y. \end{cases}$

Les deux valeurs de BD donnent evidement, 1°, $x + y = 2(F - f)$.

Pour que l'image B, donnée par la lentille a, soit vue à la distance BC: et pour que la direction du raion BHD forme un foyer relatif en D, dont le foyer equivalent soit AD; il faut (voyez l'optique de SMITH de la Traduction de l'Abbé ROCHON, p. 278. art. 245.) dis-je que $aB \times CD = AD \times BC$ savoir, 2°, $fy = Fx$.

Pour que l'objet B vû selon la direction BH, forme un foyer en D, il faut que les

lens c (namely, the diſtance φ) have this condition, $\varphi \times BD = BC \times CD$, *viz.* 3dly, $2\varphi \times \overline{F-f} = xy$.

From theſe three conditions, *viz.* $\left\{\begin{array}{ll} 1^\circ & x+y=2\times\overline{F-f}, \\ 2^\circ & fy=Fx, \\ 3^\circ & 2\varphi\times\overline{F-f}=xy, \end{array}\right\}$ we eaſily and inconteſtably find what follows:

$BC=x=\frac{2f\times\overline{F-f}}{F+f}$ the diſt. from the focus B to the lens CH,

$CD=y=\frac{2F\times\overline{F-f}}{F+f}$ the diſt. of the relative focus D, with reſpect to the two lenſes *a* and c,

$\varphi=\frac{2Ff\times\overline{F-f}}{\overline{F+f}^2}$ the focal diſtance of the lens c.

This ſolution juſt found is general; but to adapt it to a particular caſe, which may be proper for practice, I ſhall inveſtigate what relation ought to take place between the diſtances F and f when φ is $=f$. This ſuppoſition

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le foyer de la lentille c (ſavoir le foyer φ) ait cette condition ci $\varphi \times BD = BC \times CD$ ſavoir, 3°, $2\varphi(F-f)=xy$.

Avec ces trois conditions, ſavoir, $\left\{\begin{array}{ll} 1^\circ & x+y=2(F-f), \\ 2^\circ & fy=Fx, \\ 3^\circ & 2\varphi(F-f)=xy, \end{array}\right\}$ on trouve facile et inconteſtablement ce qui ſuit.

$BC=x=\frac{2f(F-f)}{F+f}$ diſtance du foyer B à la lentille CH.

$CD=y=\frac{2f(F-f)}{F+f}$ le foyer relatif D, à l'égard des deux lentilles *a* et c.

$\varphi=\frac{2Ff(F-f)}{(F+f)^2}$ foyer de la lentille c.

Cette ſolution qu'on vient de trouver eſt generale. Mais afin d'adopter un cas particulier, qui ſoit commode à pratiquer, je vais chercher les rapports, que

doivent

fition gives $\varphi = \frac{2Ff \times F-f}{\overline{F-f}^2} = f$; from which we eaſily and inconteſtably extract the relation ſought, *videlicet*, $F = \sqrt{5+2} \times f$; or this, which comes to the ſame thing, $f = (\sqrt{5-2}) \times F$.

But { $\sqrt{5+2} = 4,2361$ / $\sqrt{5-2} = 0,2361$ } { Therefore for the caſe in which $\varphi = f$, we have } { $F = 4,2361\,f$ / $f = 0,2361\,F$ } { The relation of the focal diſt.

The

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doivent avoir entre eux les foyers F et f dans le cas où l'on auroit $\varphi = f$. Cette ſuppoſition donne $\varphi = \frac{2Ff(F-f)}{(F+f)^2} = f$; d'où l'on tire facilement et inconteſtament le rapport cherché que voici $F = (\sqrt{5}+2)f$; ou bien celui ci qui revient au même que le precedent $f = (\sqrt{5}-2)F$.

Mais { $\sqrt{5+2} = 4,2361$ / $\sqrt{5-2} = 0,2361$ } { donc pour les cas où $\varphi = f$, on a } { $F = 4,2361\,f$ / $f = 0,2361\,F$ } rapport des foyers.

Application

The Application of the general Formula to the particular case of the equal lenſes a *and* c.

Let $AD = F$, the focal diſtance of the lens A,

The relation $f = 0{,}2361\ F$, found for the focal diſtances, gives

- $aB = f = 0{,}2361\ F$, the focal diſt. of the lenſes *a* and c,
- $aA = F - f = 0{,}7639\ F$, the diſt. between theſe lenſes,
- $AB = F - 2f = 0{,}5278\ F$, diſtance,
- $BD = 2 \times \overline{F - f} = 1{,}5278\ F$, diſtance,
- $BC = \frac{2f \times \overline{F - f}}{F + f} = 0{,}2918\ F$, diſtance,
- $CD = \frac{2F \times \overline{F - f}}{F + f} = 1{,}2360\ F$, diſtance.

The

Application de la formule generale au cas particulier des lentilles egales a *and* c.

Soit $AD = F$, foyer de la lentille A,

Le rapport trouvé des foyers $f = 0{,}2361\ F$, donne

- $aB = f = 0{,}2361\ F$, foyer des lentilles *a* and c.
- $aA = F - f = 0{,}7639\ F$, diſtance entre ces lentilles,
- $AB = F - 2f = 0{,}5278\ F$, diſtance,
- $BD = 2(F - f) = 1{,}5728\ F$, diſtance,
- $BC = \frac{2f(F - f)}{F + f} = 0{,}2918\ F$, diſtance,
- $CD = \frac{2F(F - f)}{F + f} = 1{,}2360\ F$, diſtance.

Application

The numerical application of the particular caſe of the equal lenſes a *and* c.

			Ft.	In.
Let	AD=F=1728 lines,	=	12	0
Then we ſhall have	aB=0,23611 F=408 lines,		2	10
	AB=0,5278 F=912 lines,		6	4
	aA=0,7639 F=1320 lines,		9	2
	aD=aB+BD=1,7639 F=3048 lines,		21	2

It is from this particular caſe, in which $\varphi=f$, and $f=F\times\sqrt{5-2}=0,23607$ F, that the following table is conſtructed.

Application numerique du cas particulier des lentilles égales a *et* c.

			P.	P.
Soit	AD=F=1728 lignes	=	12	0
Alors on aura	aB=0,23611 F=408 lignes,		2	10
	AB=0,5278 F=912 lignes,		6	4
	aA=0,7639 F=1320 lignes,		9	2
	aD=aB+BD=1,7639 F=3048 lignes,		21	2

C'eſt d'après ce cas particulier, où l'on ſuppoſe $\varphi=f$, et que $f=F(\sqrt{5}-2)=0,23607$ F, qu'eſt conſtruite la table qui ſuit.

Focal distance of the lens A and the equivalent AD.		Focal distances of the lenses *a* and c_2, and distance cA from the 2d lens c to the third A.			Distance *ac* from the first lens *a* to the second c.			Distance *aA* from the first lens *a* to third A.			Whole distance *a* D.			Distance on from the 2d lens c to the focus D.		
Fr. ft.	In.	Fr. ft.	In.	Lin.	Fr. ft.	In.	Lin.	Fr. ft.	In.	Lin.	Fr. ft.	In.	Lin.	Fr. ft.	In.	Lin.
0	1	0	0	3	0	0	6	0	0	9	0	1	9	0	1	3
0	2	0	0	6	0	1	1	0	1	6	0	3	6	0	2	6
0	3	0	0	8	0	1	7	0	2	3	0	5	3	0	3	$8\frac{1}{2}$
0	4	0	0	11	0	2	1	0	3	1	0	7	1	0	4	11
0	5	0	1	2	0	2	8	0	3	10	0	8	10	0	6	2
0	6	0	1	5	0	3	2	0	4	7	0	10	7	0	7	5
0	7	0	1	8	0	3	8	0	5	4	1	0	4	0	8	8
0	8	0	1	11	0	4	3	0	6	1	1	2	1	0	9	11
0	9	0	2	$1\frac{1}{2}$	0	4	9	0	6	$10\frac{1}{2}$	1	3	$10\frac{1}{2}$	0	11	1
0	10	0	2	$4\frac{1}{2}$	0	5	3	0	7	8	1	5	8	1	0	4
0	11	0	2	7	0	5	10	0	8	5	1	7	5	1	1	7
1	0	0	2	10	0	6	4	0	9	2	1	9	2	1	2	10
2	0	0	5	8	1	0	8	1	6	4	3	6	4	2	5	8
3	0	0	8	6	1	7	0	2	3	6	5	3	6	3	8	6
4	0	0	11	4	2	1	4	3	0	8	7	0	8	4	11	4
5	0	1	2	2	2	7	8	3	9	10	8	9	10	6	2	2
6	0	1	5	0	3	2	0	4	7	0	10	7	0	7	5	0
7	0	1	7	10	3	8	4	5	4	2	12	4	2	8	7	10
8	0	1	10	8	4	2	8	6	1	4	14	1	4	9	10	8
9	0	2	1	6	4	9	0	6	10	6	15	10	6	11	1	6
10	0	2	4	4	5	3	4	7	7	8	17	7	8	12	4	4
11	0	2	7	2	5	9	8	8	4	10	19	4	10	13	7	2
12	0	2	10	0	6	4	0	9	2	0	21	2	0	14	10	0

XIV. *Account of the Organs of Speech of the* Orang Outang. *By* Peter Camper, *M. D. late Professor of Anatomy, &c. in the University of* Groningen, *and F. R. S. in a Letter to Sir* John Pringle, *F. R. S.*

Read January 7, 1779.

Klein-Lankum, near Franequer,
Dec. 2, 1778.

SIR,

BEING lately informed by Dr. POEMMERING, whom on account of his singular industry and talents I have recommended to your favour, that you, as well as l'Abbé FONTANA and Dr. INGENHOUSZ, were surprized to hear from M. FEBRONI, the keeper of the Duke of Tuscany's Museum, that I had discovered the true organical reason for which the Orang Outang, and several other apes and monkies, are unable to speak; I take the liberty of addressing to you this anatomical essay upon the organ of speech of the Orang Outang and other monkies, in hopes you will judge it worthy to be read to the Royal Society; in whose most valuable Transactions I should be very proud to see these observations; the rather, as it is the first essay I have offered to that respectable body.

It is this diſcovery (of which I have already given a hint at the end of my Obſervations on the Rein-Deer) which Profeſſor ALLAMAND has inſerted amongſt his additions to the XVth volume of the Count DE BUFFON's Nat. Hiſt. printed at Amſterdam 1771, for S. H. SCHNEIDER, p. 55, 56. Profeſſor ALLAMAND has moreover added, in the ſame volume, p. 76: *M. le Prof.* CAMPER *a obſervé les organes de la voix de l'Orang; il s'eſt convaincu qu'il étoit impoſſible, qu'il formât des tons articulés, comme les hommes en peuvent former*, &c.

It is aſſerted by a great many travellers, that though the Orang Outang does not ſpeak, he would be able to articulate if he choſe it. Several Naturaliſts ſeem to leave this queſtion undetermined, from not having had the opportunity of diſſecting this very uncommon animal; others again overlook it, being deeply engaged in the reſearches of other parts.

My principal ſtudy in this eſſay will be to prove the abſolute impoſſibility there is for the Orang and other monkies to ſpeak.

§ 1. Being Profeſſor of Natural Philoſophy, Anatomy, Surgery, and Phyſic, in the Univerſity of Franeker in Frieſland, I ſoon perceived the impoſſibility of underſtanding the moſt precious and valuable works of the immortal GALEN (eſpecially his anatomical works) without

without diſſecting monkies, to compare his exact deſcriptions with. I got for that purpoſe, in the year 1754, a Cynocephalus, and was charmed to find the exactneſs of almoſt all GALEN's deſcriptions. The organ of ſpeech puzzled me, nevertheleſs, very much, and I was not able to explain his obſervations, ſo as to ſatisfy myſelf in this animal: I was obliged, therefore, for want of other apes, to delay my reſearches to another opportunity, which, however, I did not meet with till I came to Amſterdam, where I ſettled in the year 1755.

I diſcovered, at the beginning of 1757, in another Cynocephalus, that the baſis of the *os hyoides* was very large and hollow; and that a membranous bag, lying under the *latiſſimi colli* (which touch one another in the middle of the neck in theſe animals) went up into this bony cavity, having a communication with the inſide of the *larynx* by a hole at the root of the *epiglottis*.

The ſtructure of theſe and the Orang will be better underſtood by the four figures engraved after my own drawings for the anatomy of the Orang. I intend to publiſh them with that of the Rhinoceros with the double horn, and the Rein-Deer.

In the Cynocephalus I found the whole organ of voice pretty much like that of dogs, except the pouch *d*, *n*, *o*, *i*, fig. 4. Examining the root of the *epiglottis* I found a hole

hole *i*, *p*, fig. 3. being the real orifice of the bag *d*, *i*, *o*, *n*, fig. 4.

As all this lies above the *rima glottidis i*, fig. 3. or *i*, *h*, fig. 4. I concluded, that the voice, having paſſed the *glottis*, entered this membranous bag, *d*, *n*, *o*, *i*, by which the neck ſwelled, and out of which the air was forced by the contraction of the *latiſſimi colli*. I had often obſerved this ſwelling in ſome living apes, but now found out the reaſon of it, and was perſuaded of the incapacity of this animal to modulate his voice ſo as to articulate words.

I then conſidered this remarkable paſſage of GALEN's, de Uſu Part. Ed. Charter. tom. IV. lib. 7. cap. XI. p. 461. *Foramen in utraque linguæ* (epiglottidis) *parte unum effecit natura, et foramini ipſi parte internâ ventriculum ſuppoſuit non parvum. In quem quum aer vias nactus amplas in animal ingreditur, rurſuſque exit, nihil in ventrem depellitur*; and what he, p. 466. further obſerves, *fiſſuram potius, quàm foramen eſſe.*

When I compared this with the organ in the Cynocephali, fig. 3, 4. I was at a loſs how to explain GALEN; for I could by no means apply thoſe *ventriculi*, by which he ſeemed to have underſtood large capacities, to the ſmall holes *b*, *i*, *k*, fig. 4. above the *rima glottidis i*, *b*, which, though much larger in the Cynocephali than in

men, could not be applied to this very particular definition of no ſmall bags, *ventriculum non parvum*.

In November 1758 I diſſected another monkey, in which the membranous bag, *d*, *n*, *o*, was much larger, ſo as to occupy almoſt the whole fore part of the neck, under the *latiſſimi colli*.

In the *Apella* (the 29th ſpecies of LINNÆUS, Syſt. Nat. ed. XII. p. 42. or *ſimia caudata imberbis, cauda ſubprehenſili, corpore fuſco, pedibus nigris*) was no ſuch a bag at all, nor any opening at the root of the *epiglottis*, which was entirely ſimilar to that of dogs. I cannot forbear to mention, that in this monkey the *meatus*, or the *proceſſus peritonæi*, were cloſed as in men. This I diſſected in the year 1768, when I was Profeſſor in the Univerſity of Groningen.

Here I got the opportunity, the year following, of diſſecting two *papiones* or *ſphinges* of LINNÆUS, *ſimiæ ſemi caudatæ ore vibriſſato unguibus acuminatis*, ſpec. 6. p. 35. a male and a female; in which the *epiglottis* was likewiſe perforated, the *os hyoides* as in the former, but the pouch very ſmall in compariſon of the apes, who were very large.

As theſe parts are ſo apparent in a great many monkies, and likewiſe in the ape or *Pithecos*, I was very much ſurprized EUSTACHIUS did not diſcover them, eſpecially

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as he had taken great pains to pursue the anatomical doctrine of GALEN, as appears in the XLIST plate, where he has given several figures of this organ. I was no less surprized that ALBINUS and MARTINS did not find this bag; and I wondered likewise very much, how Mr. D'AUBENTON, who has had the greatest opportunity of any anatomist, could pass over so striking a construction of this organ. I do not mention RIOLANUS, FALLOPIUS, GORTER, SYLVIUS, BLASIUS, and some others, because they had fixed their attention upon quite different parts.

§. 2. As GALEN not only dissected the *Cebi*, or the Cynocephalus, who are all of the tailed or *caudati* kind, but the *Pithecos* or ape without a tail; and as the celebrated Dr. TYSON had found the organ of voice so similar to that of men in his *Pigmy*, I endeavoured to get one from the East Indies. For this purpose I offered a good sum of money to my correspondents to have a well-preserved Orang Outang, because none were to be met with in any collection of Natural History in Holland.

I soon got a female one in the year 1770, by the kindness and generosity of Dr. HOFFMAN, physician at Batavia, formerly my pupil; and the year 1771 another, by favour of Mr. HOPE, Director of the East India Company of Amsterdam, and Representative of his Most Serene Highness the Prince of Orange in the same Company,

who was ſo good as to order not only a female, but a male one for my uſe; but this laſt was unluckily loſt, with the whole ſhip, betwixt Java and the Cape of Good Hope.

Theſe and the ſucceeding years were very favourable to Naturaliſts; for Profeſſor ALLAMAND got a male Orang for the Muſeum of the Univerſity of Leyden; Mr. VANDER MEULEN, a great admirer of natural hiſtory (who has one of the fineſt collections at Amſterdam) received one, and Mr. VOSMAER got two for the celebrated collection of the Prince of Orange, all females. In the year 1777, Dr. VAN HOEY (a phyſician of great celebrity at the Hague, who has a very rich collection of natural curioſities) got a male (but a very young) Orang. Upon the whole, I had an opportunity of ſeeing ſeven Orangs, beſides the living one, which was ſent to his Highneſs the Prince of Orange.

All theſe reſembled perfectly in ſhape and colour that of Mr. EDWARDS, which is ſtill preſerved in the Britiſh Muſeum.

Seven of thoſe, I had ſeen, had no nails upon the great toes of the feet: it ſurprized me, therefore, to ſee them ſo diſtinctly repreſented by Profeſſor ALLAMAND. I took the liberty to inform him of it; he corrected his deſcription accordingly, p. 75. *ib. in fine*, which was eaſily

done, as the sheets were not worked off at the press. I wrote likewise to Dr. KOOYSTRA, physician to the London Infirmary, to inquire about the Orang in the British Museum. The late Dr. MATY examined, at my request, the Orang with him; and both declared, that not a single mark of a nail was to be found upon the large toes of that specimen, though Mr. EDWARDS had represented them on his 213th table so very large. These two instances shew us, how little we can depend upon figures, if not drawn with great exactness.

The want of these nails, and of the second phalanx of the large toes, is beyond any doubt a very remarkable character in this animal. Nature, however, seems to be inconstant sometimes; for, upon the great toe of the right foot of the Orang in Dr. VAN HOEY's collection, there was a little nail and two phalanges. The singular red, long hair, and the shortness of the neck, form another very peculiar property; for in the living, as well as in all the rest, I have observed the shoulders to rise up to the ears; the lower and upper jaws were much projected forwards, as I shall shew in the anatomical description of the Orang. The country they all came from was Borneo, from which island they are first sent over to Java, and so to Holland by the Cape of Good Hope.

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The Orang Outangs deſcribed by TULPIUS and TYSON came from Angola, and had both black hair, and large nails upon the great toes. I own the figures of theſe great men are very deficient in many reſpects: but, upon the whole, the animals are repreſented and deſcribed as very ſtrong and muſcular; whereas all the Orangs from Borneo, I have ſeen, were the contrary, and had long and very lean arms and legs.

To conclude, it ſeems very probable, that Africa furniſhes a peculiar ſort of apes which are not the *Pithecos* of the ancients, though theſe are not uncommon in Angola.

The organs of voice of the Angoleſe Orang, diſſected by TYSON, are very different from thoſe of the *Pithecos* which I diſſected 1777. This one had the *os hyoides* like all the *papiones* or *ſphinges*, &c.; the *epiglottis* perforated as in fig. 3. and 4. and therefore different from GALEN's deſcription, and from TYSON's, who makes no mention at all of the one, nor of the two bags which GALEN deſcribes, and which I found in the real Orang of Borneo; not only in one ſpecimen, but in five, which I have diſſected for that purpoſe.

To return to GALEN; I am very apt to think that he diſſected an Aſiatic Orang, from whom he took his deſcription of the ventricles *a latere linguæ*, at the ſides of the *epiglottis*; at leaſt that he diſſected ſuch an organ, for

the bones of the *carpus* do not entirely agree with his description, though he seems to have been very exact and nice in his dissections. And indeed I wondered as often as I compared the structure of the *carpus* and *tarsus* of apes, monkies, and dogs, with GALEN's *osteological* performances upon this subject: for though he describes but eight bones in the *carpus*, he mentions the ninth, which I have met with in all monkies, apes, and dogs, and likewise in the Orang. The tenth is not easily seen, it being very much attached to the *os naviculare*. These bones I shall give the explanation of in the anatomical description of the Orang.

In the Angolese Orang, Dr. TYSON met with the vermicular process of the *intest. cœcum*, which I found very considerable in the Asiatic; but of which GALEN appears not to have had the least notion. Mr. D'AUBENTON has given the description and figure of the same little gut in the Gibbon, a species approaching to that of the Orang, and likewise an inhabitant of Asia, but also unknown to GALEN.

§ 3. I shall now proceed to the organ of speech itself, and describe it as it appeared in the first Orang I dissected in October 1770. And for the clearer understanding I shall add some figures to it; first, of the fore-part;

ſecondly, of the *larynx* from the inſide of the *pharynx*; and, laſtly, of the inſide of the *larynx* itſelf.

Fig. 2. N, O, P, repreſents the *os hyoides*; N, O, the baſis; P the left *cornu*; N, O, the little graniform bones.

Q, T, U, the *thyroid* cartilage; V the *aſpera arteria*.

Z, X the right ventricle cut open; R, S the left.

Y the hole leading into the bag. The ventricles form here a kind of *meatus*, paſſing over the brim of the *thyroid* cartilage, under the *os hyoides*, towards the inſide, where they form the fiſſures *a*, *b*, and *a*, *i*, fig. 5. I diſſected this in the Anatomical Theatre of the Univerſity at Groningen in public.

In a ſecond, which Mr. VOSMAER, the keeper of the Prince of Orange's cabinet, was ſo kind as to grant me for diſſection, I found both theſe ventricles the ſame in every reſpect as the others, except that theſe laſt were of equal ſize.

In the third, which I diſſected at the houſe of Dr. VAN HOEY at the Hague, the 31ſt of Auguſt, 1777, the two ventricles were ſmaller but of equal ſize on both ſides. The animal was very young. The doctor preſerves this preparation in his muſeum.

In a fourth, which I preſerve intire in my collection, but whoſe organ of voice I examined the 30th of November

vember last, I found both the ventricles united so as to form but one.

The 6th figure gives a sketch of it; *a*, *c*, *d*, *e*, *f*, *g*, *h*, *b*, is the ventricle, having, nevertheless, the two *meatus's* *a* and *b*, and shewing evidently a kind of division in *i*; *g*, *h*, making a smaller bag.

This bag descended downwards to the middle of the breast bone, and spread itself sidewards over the *sterno-mastoideus*, with appendices underneath the *cucullares*.

The *latissimi colli* adhered very much to the fore-part, but sidewards; and under, from the muscles of the neck, they were easily separated by tearing gently, either with the top of the finger, or with the flat part of the handle of a dissecting knife.

As this Orang was much larger than the former ones, and consequently older, I dare not venture to determine, whether these ventricles or bags, which touch each other in the middle, grow together, so as to make but one bladder; or whether this may be a variety: because in the Orang which was alive at the Hague, and the history of which I shall give by and by, there was likewise but one bag still larger than these, and proceeding far over the *clavicles*, backwards under the *cucullares*, and before down two-thirds of the breast bone.

This accidental union can probably make no eſſential variety; for as theſe receptacles of air do not ſeem to ſerve for any modulation of voice, they will anſwer their proper purpoſe, whether united into one, or divided into two cavities. We very often ſee the kidnies united at the lower ends acroſs the ſpine in men, without its occaſioning any diſturbance in the ſecretion or animal economy.

§ 4. I muſt now give the hiſtory of the ſo much celebrated Orang which belonged to the Prince of Orange, and died in January 1777. This was a female; when alive the head was always deep in the ſhoulders, and the animal ſeldom lifted it very high up. The man who took care of her obſerved a great quantity of air under the ſkin of the neck on both ſides, which (being ignorant of theſe ventricles) he took for a dangerous diſorder, and the ſymptoms of approaching death. I felt the neck myſelf in December 1776, and diſcovered the bags to be much larger than any I had diſſected. I could remove the air eaſily with my hand from one ſide to the other, and divide it, as it were, into two parts. The bags appeared ſometimes very turgid, ſometimes collapſed.

She died not long afterwards, and was ſoon cut to pieces by the order of Mr. VOSMAER, to be ſtuffed for the Muſeum of his Serene Highneſs the Prince of Orange; but, as this cannot be done without preſerving the face, a part of the ſkull, hands, and feet, it is very evident, that Mr. VOSMAER

was

was obliged to cut off the head and the other extremities, and to destroy the most interesting parts for natural knowledge.

I was very sorry to hear the fate of this curious and uncommon creature, more especially as I had great reason to flatter myself with the dissection of the entire animal as soon as it was dead.

I need not remind any one of the particulars mentioned by Mr. FORSTER in the 2d volume of a Voyage round the World, p. 553.; nor of his rather too severe criticism upon the conduct of Mr. VOSMAER, the inspector of the Museum belonging to the hereditary Stadholder of the United Provinces. Mr. VOSMAER had, without doubt, no other intention but to preserve the fresh skin of this uncommon animal stuffed, for the cabinet of his benefactor, and not the least malevolent intention to prevent the dissection of the other parts not necessary for this purpose: for, when, by a special order of his Most Serene Highness the Prince of Orange, the remaining trunk was sent to me, I found the organ of voice not in the least hurt, and quite entire, as it is still to my great satisfaction. After having duly examined, dissected, and delineated the *viscera* of the breast and belly, I have put it in melasses, in a fine phial, in order to preserve so valuable a preparation, not only for my museum, but for natural knowledge in general.

There

There was no difference betwixt this organ and that [1] delineated in the 6th figure, but in extent. The united ventricles covered the greateſt part of the breaſt bone, and had ſeveral appendices, which inſinuated themſelves into all the interſtices of the muſcles of the neck and ſhoulders.

It had likewiſe two diſtinct *meatus's* coming from the inſide of the organ at the ſides of the *epiglottis*, as in fig. 5. and paſſing between the *os hyoides* and *thyroid* cartilage.

A large and vermicular proceſs was attached to the *cæcum*; but the inteſtines were very different on the inſide from thoſe of men. The *os femoris* was kept in its ſocket only by a ſtrong capſular ligament, there being no *ligamentum teres*: I had not obſerved one in the Orang which I preſerve, and whoſe feet I diſſected, to compare them with GALEN and others.

§ 5. Having given the ſtructure of the organ of voice in five different Orangs, and demonſtrated their conformity in every other reſpect but the union I mentioned in ſome, I ſhall proceed now to the internal part of the organ, as it is deſcribed by GALEN.

Fig. 5. ſhews the inſide of the organ, which is repreſented in fig. 2. conſequently of the ſame Orang.

d, e, f, is the *epiglottis* or *lingula*.

g, b, k, l, the *cricoid* cartilage, divided in the middle, and expanded sidewards.

b, d, h, and *g, i, f,* the *arytenoid* cartilages.

i, a, and *a, b,* the holes or fissures at each side of the *epiglottis*; *b* and *i,* the cords which form the *rima glottidis*. All this answers exactly to the description given by GALEN.

The air which is forced by expiration out of the lungs, and passes with an accelerated velocity the *rima glottidis, b, i,* being stopped by the hollow *epiglottis* and the roof of the mouth, narrow nostrils, &c. rushes into these ventricles Z, X and Q, R, S fig. 2. or into the united large ventricle *a, b, d, e, f, g,* fig. 6. These are, as GALEN rightly observes, seemingly within the animal; for they are covered with the external integuments and the *latissimi colli*. From thence, or out of these ventricles, the air gets out again by the same fissures *a, i, a, b,* fig. 5. through the mouth and nostrils, without entering into the lungs again, or, as he expresses it, without entering into the belly of the animal, *rursusque exit, nihil in ventrem depellitur*; by *venter* is to be understood the inside of the body.

If this organ does not answer entirely to the description of GALEN, I do not know how to explain the quotation; for there is no animal, as yet known, whose organ

of

of ſpeech is more applicable to it, at leaſt none of the monkey kind, as I obſerved before.

It is hardly to be conceived, how Dr. TYSON ſhould have overlooked all this, and have pronounced the organ of voice of his pigmy to be exactly like that of men, as he has done p. 51.; and yet, it is not impoſſible, when we conſider that he has overlooked other and more ſtriking differences in his eſſay.

Nor is it probable that GALEN ſhould have overlooked the large vermicular proceſs of the *cœcum* and other things, if he had diſſected the ſame kind of African Orang as TYSON did, unleſs he diſſected the Organ of voice in the one, neglecting the inteſtines, and again the bones of the feet in another, as is often the caſe with anatomiſts, as I know by my own experience. This, however, ſeems probable, that GALEN diſſected more than one ſpecies of *pithecos* or apes without a tail, and that even that ſpecies was different from the Angoleſe pigmy and from the Orang of Borneo.

§ 6. Having diſſected the whole organ of voice in the Orang, in apes, and ſeveral monkies, I have a right to conclude, that Orangs and apes are not made to modulate the voice like men: for the air paſſing by the *rima glottidis* is immediately loſt in the ventricles or ventricle of the neck, as in apes and monkies, and muſt conſe-

quently return from thence without any force and melody within the throat and mouth of these creatures: and this seems to me the most evident proof of the incapacity of Orangs, apes, and monkies, to utter any modulated voice, as indeed they never have been observed to do.

I have already mentioned in the anatomical description of the Rein-Deer (p. 55. of Mr. ALLAMAND's addition to the XVth volume of the Count DE BUFFON) the surprizing analogy of its organ of voice with that of the *Pitheci* and *Cercopitheci*. That of the Orang seems to have some analogy with that of frogs, whose voice, however, can be better modulated by their tongues. I have given a description of them in the Memoirs of the Society of Rotterdam. As I am afraid of having dwelt already too long on this subject, I shall here finish this essay; but promise to send the Society an account of the very extraordinary organ of voice of the *alouate* or *hurleur de Cayenne*, the *Simia Capucina* of LINNÆUS, *n.* 30. p. 42. the organ and *os hyoides* of which, &c. I dissected some time ago with all possible care and attention.

I am, &c.

EXPLA-

EXPLANATION OF THE FIGURES.

Fig. 1. repreſents the *pharynx* of the Orang Outang, from which the organs of voice, fig. 2. and 5. are delineated.

A, D, B, C, the tongue from behind.

D, B, C, M, the *palatum molle*, on the back part of which the *uvula* B, L is ſeen.

B, E, G, H, F, the *pharynx*, divided lengthways in the middle from E to G.

K, M, J, the paſſage from the mouth into the *œſophagus* S, K, G, H. Within this is ſeen the *epiglottis* and the *glottis* ſhut by the *arytenoid* cartilages.

Fig. 2. is the ſame organ of voice from the fore-part.

N, O, P, the *os hyoides*; N and O the little graniform bones; P, the left *cornu*.

Q, T, U, the *thyroid* cartilage; V the *aſpera arteria*.

R, S, the left ventricle intire.

W the right, cut open to ſee the orifice of the duct Y from the bag.

Fig. 3. The back part of the tongue and the *glottis* of a monkey.

a, b,

a, *b*, the *epiglottis*; *a*, *b*, *r*, *g*, the root and back part of the tongue; *c*, *s*, *t*, *d*, the *œsophagus* laid open.

u, the *aspera arteria*.

f, *e*, the *capitella* of the *arytenoid* cartilages.

e, the upper part or top of the little cartilage between the *arytenoid* cartilage and the *epiglottis*, which I have likewise met with in men, but less prominent.

i, *f*, the *rima glottidis*.

p, *i*, the hole at the root of the *epiglottis*.

Fig. 4. the inside of the *larynx* in profil.

a, *b*, *c*, *d*, the *epiglottis*; *e*, the cartilage mentioned in fig. 3.

f, *b*, the *arytenoid* cartilage; *f*, *g*, the *capitellums* forming a kind of crooked hook.

i, *m*, the cord of the *glottis*.

i, *k*, *h*, the lateral *sinus* above the *rima glottidis*, forming a pretty large ventricle in these animals.

i, *m*, *l*, the *cricoid* cartilage.

d, *n*, *o*, the ventricle, into which the air, coming through the hole at the root of the *epiglottis*, enters.

Fig. 5. the same *larynx*, represented in fig. 1. opened, to see the inward parts.

a, the union of the cords forming the *rima glottidis*.

BASIRE. SC.

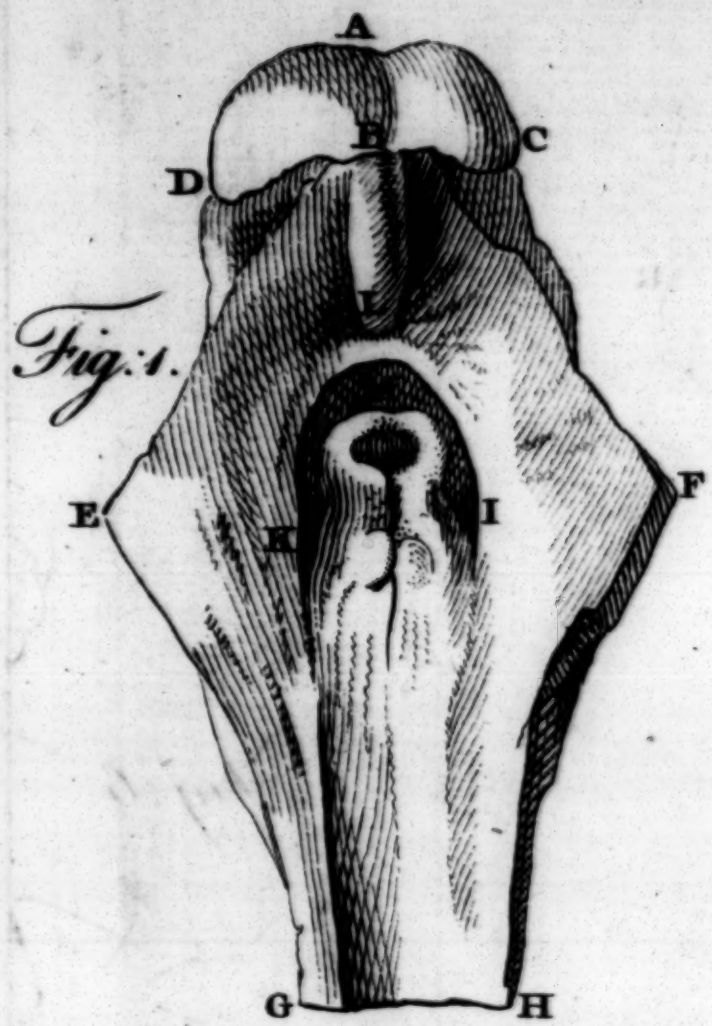


Fig: 1.

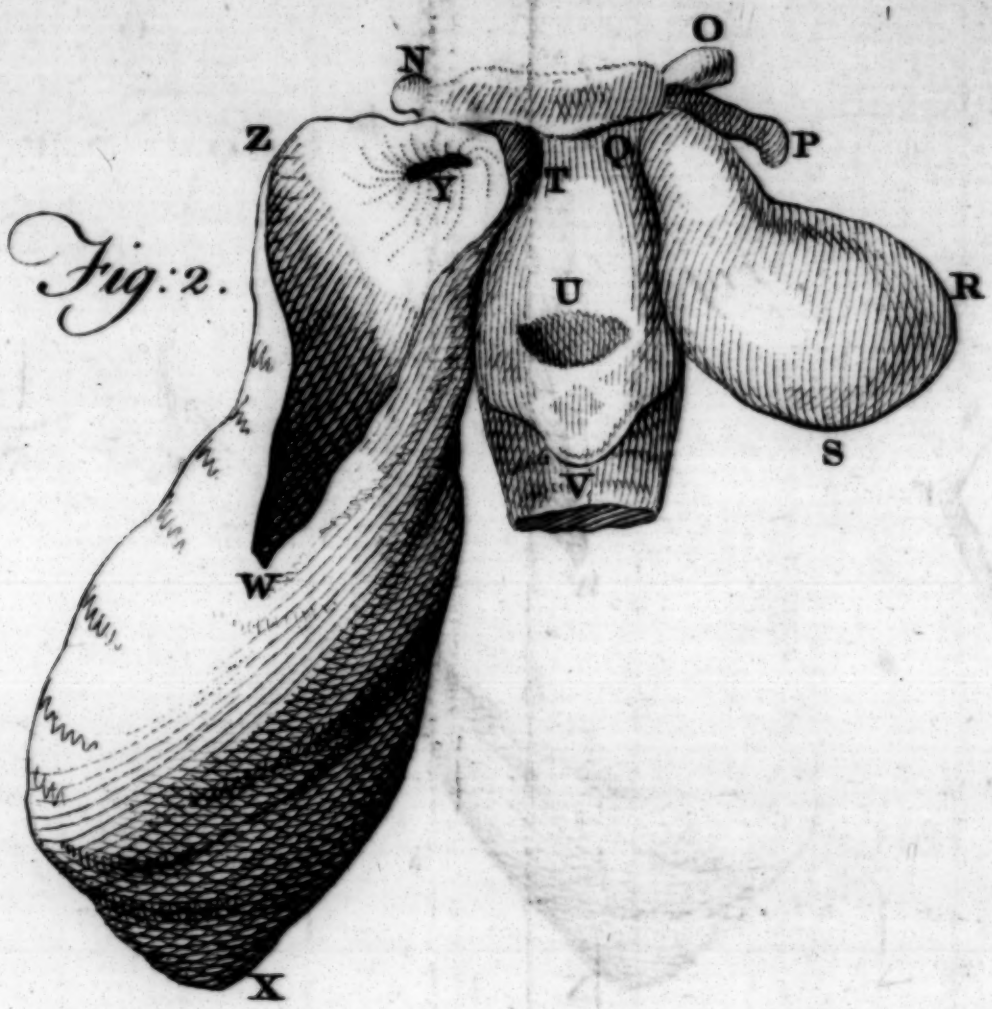


Fig: 2.

Fig: 3.

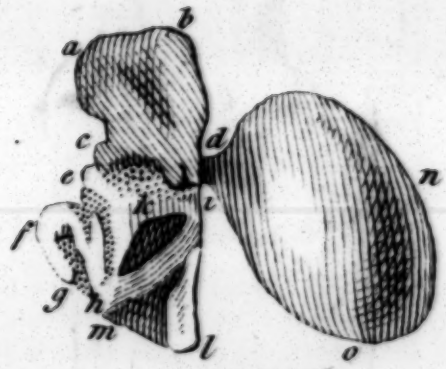
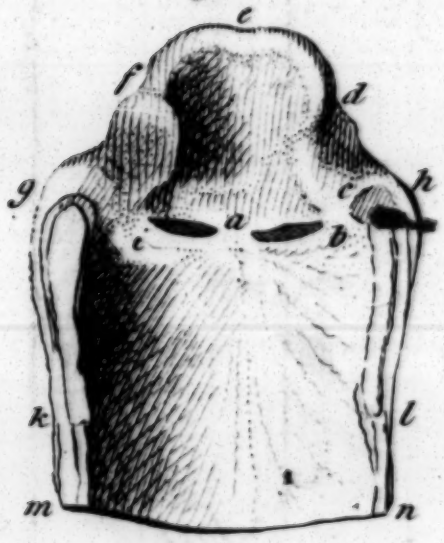


Fig: 4.

Fig: 5.



A. TIPLA. DEL.

Fig: 6.

BASIRE. SC.

a, *b*, *a*, *i*, the holes or orifices by which the air enters into the two ventricles R, S and Z, X, fig. 2.

b, *c*, *h*, *d*, the right *arytenoid* cartilage, with its *capitellum d.*

i, *g*, *f*, the left *arytenoid* cartilage.

f, *e*, *d*, the *epiglottis.*

g, *h*, *k*, *l*, the *cricoid* cartilage, divided and dilated.

k, *m*, *n*, *l*, the wind-pipe.

Fig. 6. the fore part of the Orang, which I preſerve entire in my muſeum. The ſkin of the neck and the *latiſſimi colli* are laid open, to ſhew the ventricles, &c.

A, B, C, the lower jaw-bone; A, D, A, E, the *genio hyoides.*

F, G, the *cornua* of the *os hyoides.*

H, I, the *thyroid* cartilage; K, the *cricoid.*

L, M, the ſub-maxillary glands.

a, *c*, *d*, *e*, *f*, *g*, *b*, *b*, the large bladder formed by the union of the two ventricles, of which *i*, *f*, is a mark.

a and *b*, the two *meatus's* entering towards the inſide of the *larynx*, betwixt the *thyroid* cartilage and the *os hyoides.*

XV. *Account of the Effects of Lightning on Board the* Atlas. *By* Allen Cooper, *Esq. Master of the* Atlas *East India-man. In a Letter to* Joseph Banks, *Esq. P. R. S.*

Read January 19, 1779.

DEAR SIR,

Mother-Bank, Jan. 4, 1779.

AS I am desirous of acquainting you with every observation made either by my friend Dr. LIND or myself during the voyage, I cannot help troubling you with an account of the melancholy effects of the lightning on board my ship the Atlas on the 31st of Dec. 1778. The morning and forenoon of that day were clear and cold, with a strong dry wind from the N.W. which by Dr. LIND's wind-gage generally sustained a column of water of two inches[a], and was so high as to prevent boats coming off to the ship. At 3 P.M. a squall from the N.N.W. came with a violence scarcely credible, attended with very heavy rain, large hail, and the most severe lightning, which struck our main-mast-head, descended down the mast or its rigging, and entered the gun-deck somewhere nigh the main hatchway. Those

(a) A force equal to lb. 10,4.

who were employed in letting down the ſheet cable (as we had then brought both our anchors a-head) received very ſmart ſhocks, and were witneſſes to the fire going out at ſeveral parts of the ſhip, and to an exploſion equal to that of a well-charged cannon, accompanied with a moſt ſulphureous ſmell which laſted all that day and night. It was not till the ſquall abated that our attention was called to the maſts, when we ſaw one of our beſt ſeamen hanging by his feet in the main catharpins ſtruck dead; another in the main-top was ſo miſerably ſcorched as to remain ſenſeleſs, and now continues in a dangerous way. The boatſwain's mate, who was nigh him, had his arm ſo much hurt by the ſhock he received, as not to recover the uſe of it for half an hour. All poſſible means were uſed to ſave the poor fellow who had received the fatal ſtroke; but there appeared no ſigns of returning life in him. His face was quite livid; and from the livid colour of the ſcorched places it appeared the lightning had entered his head, come out again on the left ſide of his neck, and ſpread itſelf down his left ſide and over his legs. The other man was ſtruck down in the main-top, his back much ſcorched, and on the inſide of his right leg the ſtocking burſt open a little below the knee. I have ſent the cloaths of the man who ſuffered, as alſo the ſtocking of the other man. Our top-gallant-maſts, at

the time they were ſtruck, had no iron work upon them[(b)]. Upon a careful examination, no viſible track of the lightning could be found upon the maſts, or any part over which it had paſſed, nor was any damage done to the ſhip, maſts, or rigging.

(b) On account of the ſeaſon of the year, in which lightning is ſo extremely unuſual, it had not been thought neceſſary to fix the conductor.

XVI. *Extracts of Three Letters from* John Longfield, *M.D. at* Corke *in* Ireland, *to the Aſtronomer Royal, containing ſome Aſtronomical Obſervations; together with the Longitude of* Corke, *deduced from the ſaid Obſervations, by the Aſtronomer Royal.*

Read February 11, 1779.

SIR, Corke, Oct. 1, 1773.

ENCOURAGED by Mr. WALSH, with whom I have the pleaſure of being acquainted, and who has favoured me with a letter to you; and being confident of your inclination to promote Geography and Aſtronomy; I trouble you with a few obſervations, and requeſt your advice and aſſiſtance in the proſecution of my favourite ſtudy.

Laſt year I built a very ſolid and commodious, though ſmall obſervatory, cloſe to my houſe. The top of one of the hills which ſurround this city would have been a more eligible ſituation; but as my profeſſion confines me to the town I had no choice. To make up its deficiency in height, I have adapted the upper part of my houſe to the

ſame purpoſe, which ſerves very well for inſtruments that do not require great ſolidity: From hence I have a very extenſive view all round. In the obſervatory is a ſolid pier, ſunk deep in the ground, on the top of which ſtands the tranſit inſtrument. It is of tin, three feet and a half long, and made by Mr. MONK. The other inſtruments I have are the following:

An equal altitude inſtrument.

A quadrant of one foot radius, by Mr. BIRD.

Another of two feet and a half radius, of the French conſtruction, not a bad one with regard to the diviſions, but inconvenient.

An excellent teleſcope, with a treble object-glaſs, by Mr. DOLLOND. It magnifies about 140 times, and is of the lateſt improvement.

A parallactic inſtrument, with a common teleſcope five feet long, and a *reticule rhomboide*.

Two aſtronomical clocks, with wooden pendulums, one of which goes full as well as Mr. WOOLLASTON's. The alterations in their going ſeem to be owing to moiſture. I conſtantly compare them with SMEATON's hygrometer.

With the aſſiſtance of a particular friend, who is an excellent mathematician and a very good obſerver [Mr. ELIAS MAINADUC], I have taken a great number of meridian

meridian altitudes of the Sun, and of ſtars to the North and South of the zenith. The mean of both quadrants makes the latitude 51° 53′ 54″.

		App. time. h	′	″	
1772, July	4 at	12	52	0	4's 2d ſat. imm. hazy.
	18 -	12	41	23	1ſt ſat. imm.
Aug.	3 -	10	58	27	ditto, ditto.
Sept.	20 -	8	21	16	ditto, emerſ.
Oct.	6 -	6	46	12	ditto, ditto, hazy.

They were all obſerved with the greateſt magnifying power.

The weather has been almoſt continually cloudy for theſe ſix months paſt, which has prevented me from making any obſervation of conſequence except on April, 29th, an occultation of 1 α Cancri by the Moon at 11h 23′ 25″ apparent time, and an immerſion of Jupiter's firſt ſatellite at 12h 52′ 29″ on Auguſt the 22d.

I ſhould be much obliged to you for the correſponding obſervations to the above, if it would not be too much trouble.

I am, &c.

SIR,

SIR, Corke, Feb. 18, 1774.

I HAVE now had a full trial of SMEATON's hygrometer, and think it a very uſeful and agreeable inſtrument. It is capable of a conſiderable degree of exactneſs, but not ſufficient for any thing which requires great accuracy. It abſorbs the moiſture of the air much more readily than it parts with it; and I have great reaſon to believe, that, as the gravity of the air differs, it will point to different degrees on the ſcale, the degree of moiſture or dryneſs being the ſame. If the cord is too much twiſted it will require frequent adjuſtments, and I know that the extreme degree of moiſture is very uncertain (though that of dryneſs is not ſo) and different perſons will adjuſt it very differently at that point.

My teleſcope I have found, by theory and experiment, to magnify 134 times.

My friend, whom I mentioned to you, has been making obſervations for theſe thirty years. He has a good clock, with a common pendulum; a quadrant of two feet radius; a five feet teleſcope of two glaſſes, with a common micrometer; an excellent reflector of SHORT's, of eighteen inches focus; and is getting a tranſit inſtrument. His

obſervations

observations gave him the latitude of Corke 51° 54′, and the longitude 34½ minutes of time West of Greenwich.

I am, &c.

1773	Clock differs from mean Time.	Gains or loses *per diem.*	Mean of Therm. in obs.	Mean of Hygr.	Observations.
	′ ″				
Sept. 11	−0 17.0		60	54	Hygr. was put into the observ. clock was losing 0.5 *per diem.*
12	−0 17.0	−0.0	60	50	
13	−0 17.5	−0.5	60	38	
14			60	36	
15	−0 18.5	−0.5	60	30	
16			60	26	
17			60	20	
18	−0 20.0	−0.5	60	16	
19			60	14	
20			60	18	
21			60	14	
22	−0 21.0	−0.25	55	14	
23			53	20	
24			60	35	
25			60	42	
26			60	35	
27			58	25	
28			58	33	
29	−0 27.0	−0.8	54	32	
30			55	32	
Oct. 1	−0 28.7	−0.8	55	40	
2			53	28	

1773	Clock differs from mean time.	Gains or loses *per diem*.	Mean of Therm. in obs.	Mean of Hygr.	Observations.
	′ ″				
Oct. 3			48	30	Hygrometer adjusted, much lengthened.
4			48	28	
5			50	20	
6	−0 33.0	−0.8	53	22	
7			50	24	
8	−0 35.0	−1.0	55	25	
9			54	22	
10	−0 37.0	−1.0	54	20	
11			53	25	
12	−0 39.0	−1.0	53	14	
13			50	15	
14			54	18	
15			50	20	
16			53	16	
17			54	18	
18			54	18	
19	−0 49.5	−1.5	54	20	
20			53	30	
21			54	42	
22			54	38	
23			53	30	
24	−0 56.5	−1.4	54	25	
25			53	24	
26			54	20	
27			56	18	
28			54	20	Hygrometer adjusted.
29			45	24	

1773	Clock differs from mean time.	Gains or loses *per diem*.	Mean of Therm. in obs.	Mean of Hygr.	Observations.
	′ ″				
Oct. 30	−1 2.5	−1.0	45	30	
31			45	36	
Nov. 1			46	40	
2			45	30	
3			45	32	
4			44	38	
5			45	35	
6			44	40	
7	−0 59.0	+0.4	48	48	
8			46	49	
9			46	59	
10			44	44	
11	−0 58.5	+0.1	44	38	
12			43	34	{ Hygr. adjusted, had lengthened 1° only.
13	−0 58.0	+0.2	46	37	
14			45	40	
15	−0 58.0	+0.0	46	34	
16			44	39	
17	−0 57.0	+0.5	42	36	
18			40	30	
19			41	28	
20	−0 55.5	+0.5	41	28	
21			45	30	
22	−0 54.5	+0.5	36	31	
23			35	32	
24	−0 52.7	+0.9	38	29	

1773	Clock differs from mean time.	Gains or loses *per diem.*	Mean of Therm in obs.	Mean of Hygr.	Observations.
	′ ″				
Nov. 25			36	28	
26	−0 51.0	+0.9	40	29	
27			43	48	
28			46	40	
29			40	33	
30	−0 49.0	+0.5	43	32	
Dec. 1	−0 48.5	+0.25	45	34	
2			47	36	
3			46	37	
4	−0 48.0	+0.1	42	32	
5	−0 47.5	+0.5	38	30	
6			40	32	
7			38	29	
8	−0 44.5	+1.0	38	29	
9			38	30	
10			45	48	
11			38	43	
12			44	45	
13	−0 39.5	+1.0	46	30	
14			48	37	
15			47	44	
16			47	42	
17			47	42	
18			47	42	
19			46		
20	−0 31.5	+1.1	48		

1773	Clock differs from mean time.	Gains or loses *per diem.*	Mean of Therm. in obs.	Mean of Hygr.	Observations.
	′ ″				
Dec. 21			50		
22			42		
23	−0 27.5	+1.3	39		
24	−0 26.0	+1.5	36		
25	−0 24.5	+1.5	34		
26			37		
27			40		
28	−0 18.5	+2.0	36		
29			37		
30	−0 15.0	+1.7	34		
31	−0 13.0	+2.0	36		
1774 Jan. 1			36		
2	−0 8.4	+2.3	32		
3			30		
4	−0 3.8	+2.3	36		
5	−0 2.0	+1.8	42		
6			43		
7			40		
8			46		
9			42		
10			41		
11			40		
12			44		
13			45		
14	+0 7.0	+1.0	43		
15			46		

1774	Clock differs from mean time.	Gains or loses *per diem.*	Mean of Therm. in ob	Mean of Hygr.	Observations.
	′ ″				
Jan. 16	+0 9.0	+1.0	48		
17			45		
18			32		
19			39	39	
20			43	45	
21			43	45	
22			43	49	
23	+0 18.5	+1.3	38	37	
24			35	30	
25			35	36	
26			42	33	
27	+0 24.5	+1.5	38	44	
28			41	34	
29	+0 26.5	+1.0	37	32	
30			34	32	
31	+0 28.5	+1.0	35	24	
Feb. 1			32	28	
2	+0 30.0	+0.7	32	24	
3			32	24	
4	+0 31.0	+0.5	33	25	
5			34	26	
6	+0 32.0	+0.5	42	26	
7			42	30	
8	+0 32.5	+0.25	37	26	
9			39	27	
10			44	47	

1774	Clock difers from mean time.	Gains or loſes *per diem.*	Mean of Therm. in obſ.	Mean of hygr.	Obſervations.
	′ ″				
Feb. 11			42	40	
12			49	50	
14	+0 33.5	+0 5	46	49	
15			41	41	
16			40	37	
17	+0 35.0	+0 5	41	30	
18	+0 35.5	+0.5	38		

My clock was bought three years ſince at an auction, among the collection of clocks of ſome gentleman, who was curious in that way in London. As ſoon as it arrived I bought it from the purchaſer, a watch-maker: the name is HUGHES. It beats dead ſeconds, goes a month, and is finiſhed both inſide and out with ſo much pains and elegance, that the clock-makers inform me, it muſt have coſt forty pound. It had a common pendulum with a heavy bob. I got a wooden pendulum and a new crutch applied to it exactly according to Mr. LUDLAM's directions, except that the ſpring is longer. For four months after it was put up it went irregularly, upon which I faſtened it to the wall with ſcrew bolts and large waſhers. Since that time, May 1772, it never loſt more than 2″ *per diem*, or gained more than 2″.5, nor did it ever differ more than 1″ in its daily rate on any two ſucceſſive days.

SIR, Corke, Feb. 17, 1779.

I SEND you all the obſervations of any conſequence that have been made here. Some of them are by Mr. NEWENHAM, a young gentleman of conſiderable abilities. He lives on a hill, about 2400 yards E. [anſwering to a difference of meridians of $7\frac{2}{3}$ ſeconds of time] and 600 N. of my obſervatory [anſwering to 18″ difference of latitude] and has a clock with a wooden pendulum; a tranſit teleſcope of 30 inches in length, with an achromatic object-glaſs; and a reflecting teleſcope of eighteen inches focus, made by DOLLOND, magnifying 70 times very diſtinctly. Mr. MAINADUC's obſervatory is 1600 feet due W. of mine [anſwering to a difference of meridian of five ſeconds of time]. The eclipſe of the Sun was obſerved there, for the convenience of the ſide-glaſs, with a tranſit teleſcope of ſmall magnifying power, placed on rack-work; ſo that, though it was a very good obſervation, the beginning may be reckoned three or four ſeconds ſooner. I was unluckily interrupted at the inſtant. The end was not ſeen. The longitude of Corke is, I believe, ſufficiently determined by the occultations. I have calculated ſome of them. If you ſhould

take

take that trouble, I ſhall be much obliged to you if you will let me know the reſult.

The account I ſent you of the going of one of my clocks, I am afraid, is not worth laying before the Royal Society, as no concluſion can be deduced from it; but I am certain, that moiſt weather, for any length of time, makes it go ſlower, probably by increaſing the weight of the rod: perhaps covering it with tin-foil would prevent its imbibing moiſture. The other clock, with a mahogany pendulum, does not go well, as the fibres of the wood are not ſtrait, and it warps from the changes of the weather.

I have taken ſome pains to fix the hygrometer to ſome ſtandard, but in vain. One I have had about five years, though adjuſted laſt ſummer, has almoſt loſt the power of abſorbing moiſture; ſo that its contracting is to its lengthening as 1 to 3.

In 1774 I ſent my achromatic teleſcope to DOLLOND and got another from him much better in every reſpect magnifying 150 times, with an achromatic object-glaſs micrometer, and a very firm ſtand and polar axis. It has but one ſet of ſliding tubes at the object end, yet it is very ſteady, and anſwers perfectly well, when once it is fixed to the object.

Being

Being at one time intent on making ſome obſervations with the croſs wires in your manner, I got a ſtand made with an arm, to carry a lanthern that ſhould follow the motion of the teleſcope, and applied a ſolid illuminator to it occaſionally. The account of this I firſt met with in the preface to your admirable obſervations.

I changed the object of my tranſit teleſcope for an achromatic one, got a ſolid illuminator to it, and put in very fine wires, by all which it is much improved.

I am, &c.

Eclipſes

Eclipses of Jupiter's first satellite.

	App. time. h ′ ″	
1772, July 18	12 41 23	Immersion, SHORT's 18 inch R. M. P. 130, by Mr. MAINADUC.
Aug. 3	10 58 27.5	Immersion, achrom. 134.
Sept. 20	8 21 16	Emersion, ditto.
Oct. 6	6 46 12	Emersion, ditto.
1773, Aug. 22	12 52 29	Immersion, ditto.
Nov. 24	10 40 16.5	Emersion, ditto.
1774, Nov. 13	10 20 27	Emersion, achrom. 150.
29	8 34 19	Emersion, ditto.
1775, Jan. 14	8 43 23	Emersion, ditto.
Mar. 1	9 13 16	Emersion, ditto.

By Mr. GEORGE NEWENHAM.

1774, Nov. 29	8 34 42	Emersion, refl. DOLLOND, 70 M. P.
Dec. 8	4 54 56	Emersion, ditto.
22	8 38 45	Emersion, ditto.
1775, Jan. 14	8 43 34	Emersion, ditto.
Sept. 29	16 47 31	Immersion, ditto.
Oct. 1	11 16 41.5	Immersion, ditto.
22	17 1 53.5	Immersion, ditto.
24	11 31 25	Immersion, ditto.

Occultations.

	App. time. h ′ ″	
1773, Apr. 29	11 23 25	1 α Cancri, immerſion.
1774, Apr. 14	6 41 49	α Tauri, emerſion.
1775, Sept. 14	11 7 1	γ Tauri, immerſion.
	20 13 12	α Tauri, emerſion, uncertain to two ſeconds.
1777, Sept. 21	10 26 39	1 δ Tauri, immerſion.
	10 43 28	2 δ Tauri, immerſion.

By Mr. NEWENHAM.

1774, Nov. 18	14 35 42	α Tauri, immerſion.
	15 52 27	α Tauri, emerſion.
1776, Jan. 8	17 59 17.5	α Leonis, immerſion.
	18 48 55.5	α Leonis, emerſion.

Eclipſe of the Sun.

1778, June 24	2 57 16	Beginning, Mr. MAINADUC.

Magnetic variation 24° W. in July 1778.

The

The Longitude of Corke *ſettled from the foregoing obſervations compared with others made at the Royal Obſervatory at* Greenwich. *By* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

THE obſervations made at the Royal Obſervatory at Greenwich neareſt to thoſe made at Corke are as follows, and the error of the Nautical Almanac with reſpect to the time obſerved is ſet down, and alſo the correction of the Nautical Almanac, with reſpect to the time obſerved, and reduced to the effect of a $3\frac{1}{2}$ feet teleſcope, which ſhews the immerſions of the firſt ſatellite ſooner, and the emerſions later, than the ſix feet reflector does by about 13″.

		Apparent time.	Teleſcope.	State of air.	Corr. of Nautical Almanac.	Correction of Naut. Almanac for $3\frac{1}{2}$ teleſcope.
		h ′ ″	Feet.		′ ″	′ ″
1772, July 11	Im.	11 22 25	$3\frac{1}{2}$	— — —	−0 11	−0 11
Aug. 26	Em.	14 4 22	6	Air clear.	−0 19	−0 6
Sept. 27	Em.	10 52 43	$3\frac{1}{2}$	Air very clear.	−0 12	−0 12
Oct. 13	Em	9 17 4	$3\frac{1}{2}$	Air clear.	−0 22	−0 22
1773, Aug. 31	Im.	9 51 57	6	Air clear.	−0 3	−0 16
1774, Sept. 10	Im.	15 28 31	6	Air clear & 4's belts diſtinct.	+0 57	+0 44
Dec. 29	Em.	11 3 48	6	— — —	−0 4	+0 9
1775, Feb. 22	Em.	7 49 37	6	Air clear.	+0 29	+0 42
Aug. 7	Im.	14 53 55	6	Air very clear.	+1 28	+1 15
Oct. 22	Im	17 37 1	6	Air very clear.	+1 33	+1 20

Hence the times of the Nautical Almanac corrected and reduced to the effect of a $3\frac{1}{2}$ feet telescope, and compared with the observations made at Corke, to find the difference of meridians of Greenwich and Corke, are as follows:

		Observed at Corke.	Corr. of Nautical Almanac.	Nautical Almanac corrected.	Diff. of meridians.	Mean.
		h ′ ″	′ ″	h ′ ″	′ ″	
1772, July 18	Im.	12 41 23	−0 11	13 15 37	34 14	*per* immersions 34′ 10″½.
1773, Aug. 22	Im.	12 52 29	−0 16	13 26 36	34 7	
1772, Sept. 20	Em.	8 21 16	−0 12	8 55 0	33 44	*per* emersions 33′ 44″.
1773, Nov. 24	Em	10 40 16	−0 8	11 14 6	33 50	
1774, Nov. 29	Em.	8 34 19	+0 9	9 8 15	33 56	
1775, Jan. 14	Em.	8 43 23	+0 9	9 17 3	33 40	
Mar. 1	Em.	9 13 16	+0 42	9 46 45	33 29	
				Mean of two results,		33′ 57″.

Hence the difference of meridians of Greenwich and Corke is 33′ 57″ of time, and the longitude of Corke is 8° 29′ 15″ West of Greenwich. The latitude of Corke, as determined by Dr. LONGFIELD, by a mean from two quadrants is 51° 53′ 54″ North.

By Mr. NEWENHAM's observations compared in like manner the difference of meridians of Greenwich and his observatory is 34′ 11″, which, according to Dr. LONGFIELD's observations, allowing 8″ for the difference of meridians,

meridians, owing to the diſtance of the two obſervatories, ſhould be 33′ 49″, which latter reſult is moſt to be depended upon. The latitude of Mr. NEWENHAM's obſervatory being 18″ greater than that of Dr. LONGFIELD's, according to the meaſured diſtance, is 51° 54′ 12″ North.

XVII. *The Latitude of* Madraſs *in the* Eaſt Indies, *deduced from Obſervations made by* William Stephens, *Chief Engineer. Communicated by* John Call, *Eſq. F. R. S.*

Read February 11, 1778.

Time.	What objects.			Latitudes of Madraſs, from Southern Z. diſt.			from Northern Z. diſt.		
1776, Oct.	4. Sun	—	—	13	3	58	–	–	
	5. Sun	—	—	13	4	20	–	–	
	19. Sun	—	—	13	4	16	–	–	
	22. Sun	—	—	13	3	56	–	–	
Nov.	1. β Caſſiopea	—	—	–		–	13	5	42
	α Caſſiopea	—	—	–		–	13	5	58
	γ Caſſiopea	—	—	–		–	13	5	44
	δ Caſſiopea	—	—	–		–	13	5	39
	2. ∂ Capricorn	—	—	13	4	29	–	–	
	Fomalhaut	—	—	13	4	29	–	–	
	β Ceti	—	–	13	3	55	–	–	
	3. Moon, this rejected in medium,			–		–	13	6	58
	10. Sun, very good,		—	13	4	9	–	–	
	ϒ Lacerta	—	—	–		–	13	5	25
	λ Andromeda (called 1λ)		—	–		–	13	5	25
	β Caſſiopea	—	—	–		–	13	5	42
	ε Caſſiopea	—	—	–		–	13	15	9
				13	4	11	13	5	37

Mean latitude 13° 4′ 54″ North.

The above obſervations were taken with an aſtronomical braſs quadrant on the top of the houſe uſually inhabited by the chief engineer.

XVIII. *Account of an Infant Muſician.* *By* Charles Burney, *Doctor of Muſic and F. R. S.*

Read February 18, 1779.

TO DR. WILLIAM HUNTER, F. R. S.

SIR,

St. Martin's-ſtreet,
Feb. 9, 1779.

AS your curioſity ſeemed much excited by the extraordinary accounts of the Norwich muſical child, and as you expreſſed ſome deſire to know in what particulars his performance was wonderful, and diſpoſition to muſic ſuperior to that of other children of the ſame age: after making all the inquiries my leiſure and opportunities would allow, and repeatedly hearing and ſtudying him, I have drawn up the following account, which, if it does not appear too trivial, I ſhould be glad you would do me the honour of preſenting to the Royal Society, as a mark of my reſpect and veneration for that learned Body, who, as their inquiries extend to all parts of Nature, will perhaps not diſdain to receive a narrative of the uncommon exertions of the human faculties at a more early period of life than they uſually develope.

I have the honour to be, &c.

THAT reaſon begins to dawn, and reflexion to operate, in ſome children much ſooner than in others, muſt be known to every one who has had an opportunity of comparing the faculties of one child with thoſe of another. It has, however, ſeldom been found, that the ſenſes, by which intelligence is communicated to the mind, advance with even pace towards perfection. The eye and the ear, for inſtance, which ſeem to afford reaſon its principal ſupplies, mature at different periods, in proportion to exerciſe and experience; and not only arrive at different degrees of perfection during the ſtages of infancy, but have different limits at every period of human life. An eye or ear that only ſerves the common purpoſes of exiſtence is intitled to no praiſe; and it is only by extraordinary proofs of quickneſs and diſcrimination in the uſe of theſe ſenſes, that an early tendency to the art of painting or muſic is diſcovered.

Many children, indeed, ſeem to recognize different forms, perſons, ſounds, and tones of voice, in very early infancy, who never afterwards endeavour to imitate forms by delineation, or ſounds by vocal inflexions.

As drawing or deſign may be called a refinement of the ſenſe of ſight, and practical muſic of that of hearing; and as a perfection in theſe arts at every period of life,

from

from the difficulty of its attainment, and the delight it affords to the admirers and judges of both, is treated with reſpect, a premature diſpoſition to either uſually excites the ſame kind of wonder as a phenomenon or prodigy.

But as perſons conſummate in theſe arts, and who are acquainted with the uſual difficulties which impede the rapid progreſs of common ſtudents, can only judge of the miraculous parts of a child's knowledge or performance, it will be neceſſary, before I ſpeak of the talents peculiar to the child who is the ſubject of the preſent inquiry, to diſtinguiſh, as far as experience and obſervation ſhall enable me, between a common and ſupernatural diſpoſition, during infancy, towards the art of muſic.

In general a child is not thought capable of profiting from the inſtructions of a muſic-maſter till five or ſix years old, though many have diſcovered an ear capable of being pleaſed with muſical tones, and a voice that could imitate them, much ſooner. The lullaby of a nurſe during the firſt months of a child's exiſtence has been found to ſubdue peeviſhneſs, and, perhaps, divert attention from pain; and in the ſecond year it has often happened, that a child has not only been more diverted with one tune or ſeries of ſounds than another, but has had ſufficient power over the organs of voice to imitate the inflexions by which it is formed; and theſe early proofs of

what is commonly called muſical genius would doubtleſs be more frequently diſcovered if experiments were made, or the mothers or nurſes were muſically curious. However, ſpontaneous efforts at forming a tune, or producing harmony upon an inſtrument ſo early, have never come to my knowledge.

The arts being governed by laws built on ſuch productions and effects as the moſt poliſhed part of mankind have long agreed to call excellent, can make but ſmall approaches towards perfection in a ſtate of nature, however favourable may be the diſpoſition of thoſe who are ſuppoſed to be gifted with an uncommon tendency towards their cultivation. Nature never built a palace, painted a picture, or made a tune: theſe are all works of art. And with reſpect to architecture and muſic, there are no models in nature which can encourage imitation: and though there is a wild kind of muſic among ſavages, where paſſion vents itſelf in lengthened tones different from thoſe of ſpeech, yet theſe rude effuſions can afford no pleaſure to a cultivated ear, nor would be honoured in Europe with any better title than the howlings of animals of an inferior order to mankind.

All therefore that is really admirable in early attempts at muſic is the power of imitation; for elegant melody and good harmony can only be ſuch as far as they correſpond

reſpond with or ſurpaſs their models: and as melody conſiſts in the happy arrangement of ſingle ſounds, and harmony in the artificial combination and ſimultaneous uſe of them, an untaught muſician becomes the inventor of both; and thoſe who are at all acquainted with the infancy of ſuch melody and harmony as conſtitute modern muſic, can alone form an idea of the rude ſtate of both when an individual diſcovers them by the ſlow proceſs of experiment.

Every art when firſt diſcovered ſeems to reſemble a rough and ſhapeleſs maſs of marble juſt hewn out of a quarry, which requires the united and ſucceſſive endeavours of many labourers to form and poliſh. The zeal and activity of a ſingle workman can do but little towards its completion; and in muſic the undirected efforts of an infant muſt be ſtill more circumſcribed: for, without the aid of reaſon and perſeverance he can only depend on memory and a premature delicacy and acuteneſs of ear for his guides; and in theſe particulars the child of whom I am going to ſpeak is truly wonderful.

WILLIAM CROTCH was born at Norwich July 5, 1775. His father, by trade a carpenter, having a paſſion for muſic, of which however he had no knowledge, undertook to build an organ, on which, as ſoon as it would ſpeak, he learned to play two or three common tunes,

ſuch as *God ſave great* GEORGE *our king*; *Let ambition fire thy mind*; and *The Eaſter Hymn*; with which, and ſuch chords as were pleaſing to his ear, he uſed to try the perfection of his inſtrument.

I have been favoured with ſeveral particulars concerning his ſon's firſt attention to muſic from ROBERT PARTRIDGE, eſquire, a gentleman of rank in the Corporation of Norwich, who, at my requeſt, has been ſo obliging as to aſcertain many curious facts, the truth of which, had they reſted merely on the authority of the child's father or mother, might have been ſuſpected; and tranſactions out of the common courſe of nature cannot be too ſcrupulouſly or minutely proved.

My correſpondent, of whoſe intelligence and veracity I have the higheſt opinion, tells me, that I may reſt aſſured of the authenticity of ſuch circumſtances as he relates from the information of the child's father, who is an ingenious mechanic, of good reputation, whom he knows very well, and frequently employs, as theſe circumſtances are confirmed by the teſtimony of many who were witneſſes of the child's early performance; and he adds, that he has himſelf ſeen and heard moſt of the very extraordinary efforts of his genius.

About Chriſtmas 1776, when the child was only a year and a half old, he diſcovered a great inclination for muſic,

muſic, by leaving even his food to attend to it when the organ was playing: and about Midſummer 1777 he would touch the key-note of his particular favourite tunes, in order to perſuade his father to play them. Soon after this, as he was unable to name theſe tunes, he would play the two or three firſt notes of them when he thought the key-note did not ſufficiently explain which he wiſhed to have played.

But, according to his mother, it ſeems to have been in conſequence of his having heard the ſuperior performance of Mrs. LULMAN, a muſical lady, who came to try his father's organ, and who not only played on it, but ſung to her own accompanyment, that he firſt attempted to play a tune himſelf: for, the ſame evening, after her departure, the child cried, and was ſo peeviſh that his mother was wholly unable to appeaſe him. At length, paſſing through the dining-room, he ſcreamed and ſtruggled violently to go to the organ, in which, when he was indulged, he eagerly beat down the keys with his little fiſts, as other children uſually do after finding themſelves able to produce a noiſe, which pleaſes them more than the artificial performance of real melody or harmony by others.

The next day, however, being left, while his mother went out, in the dining-room with his brother, a youth

of

of about fourteen years old, he would not let him reſt till he blew the bellows of the organ, while he ſat on his knee and beat down the keys, at firſt promiſcuouſly; but preſently, with one hand, he played enough of *God ſave great* GEORGE *our King* to awaken the curioſity of his father, who being in a garret, which was his work-ſhop, haſtened down ſtairs to inform himſelf who was playing this tune on the organ. When he found it was the child, he could hardly believe what he heard and ſaw. At this time he was exactly two years and three weeks old, as appears by a copy I have obtained of the regiſter in the pariſh of St. George's Colgate, Norwich, ſigned by the reverend Mr. TAPPS, miniſter. Nor can the age of this child be ſuppoſed to exceed this account by thoſe who have ſeen him, as he has not only all the appearance, but the manners, of an infant, and can no more be prevailed on to play by perſuaſion than a bird to ſing.

It is eaſy to account for *God ſave great* GEORGE *our King* being the firſt tune he attempted to play, as it was not only that which his father often performed, but had been moſt frequently adminiſtered to him as a narcotic by his mother, during the firſt year of his life. It had likewiſe been more magnificently played than he was accuſtomed to hear by Mrs. LULMAN, the afternoon before he became a practical muſician himſelf; and, previous

vious to this event, he uſed to teize his father to play this tune on his organ, and was very clamourous when he did not carry his point.

When his mother returned, the father, with a look which at once implied joy, wonder, and myſtery, deſired her to go up ſtairs with him, as he had ſomething curious to ſhew her. She obeyed, imagining that ſome acquaintance or friend was arrived, or that ſome intereſting event had happened during her abſence; but was as much ſurprized as the father on hearing the child play the firſt part of *God ſave great* GEORGE *our King*. The next day he made himſelf maſter of the treble of the ſecond part; and the day after he attempted the baſe, which he performed nearly correct in every particular, except the note immediately before the cloſe, which, being an octave below the preceding ſound, was out of the reach of his little hand.

In the beginning of November 1777 he played both the treble and baſe of *Let ambition fire thy mind*, an old tune which is, perhaps, now better known by the words to which it is ſung in Love in a Village, *Hope, thou nurſe of young deſire*.

Upon the parents relating this extraordinary circumſtance to ſome of their neighbours, they laughed at it; and, regarding it as the effect of partial fondneſs for

their

their child, adviſed them by no means to mention it, as ſuch a marvellous account would only expoſe them to ridicule. However, a few days after, Mr. CROTCH being ill, and unable to go out to work, Mr. PAUL, a maſter-weaver by whom he was employed, paſſing accidentally by the door, and hearing the organ, fancied he had been deceived, and that CROTCH had ſtayed at home in order to divert himſelf on his favourite inſtrument; fully pre-poſſeſſed with this idea, he entered the houſe, and, ſuddenly opening the dining-room door, ſaw the child playing on the organ while his brother was blowing the bellows. Mr. PAUL thought the performance ſo extraordinary, that he immediately brought two or three of the neighbours to hear it, who propagating the news, a croud of near a hundred people came the next day to hear the young performer, and, on the following days, a ſtill greater number flocked to the houſe from all quarters of the city; till, at length, the child's parents were forced to limit his exhibition to certain days and hours, in order to leſſen his fatigue, and exempt themſelves from the inconvenience of conſtant attendance on the curious multitude.

This account agrees in moſt particulars with a letter I received from Norwich, and of which the following is an extract.

"There

"There is now in this city a muſical prodigy, which "engages the converſation and excites the wonder of "every body. A boy, ſon to a carpenter, of only two "years and three quarters old, from hearing his father "play upon an organ which he is making, has diſco-"vered ſuch muſical powers as are ſcarcely credible. He "plays a variety of tunes, and has from memory re-"peated fragments of ſeveral voluntaries which he "heard Mr. GARLAND, the organiſt, play at the cathe-"dral. He has likewiſe accompanied a perſon who "played upon the flute, not only with a treble, but has "formed a baſe of his own, which to common hearers "ſeemed harmonious. If any perſon plays falſe, it "throws him into a paſſion directly; and though his "little fingers can only reach a ſixth, he often attempts "to play chords. He does not ſeem a remarkable "clever child in any other reſpect; but his whole "ſoul is abſorbed in muſic[a]. Numbers croud daily

(a) This opinion ſeems to have been too haſtily formed; for, independent of his muſical talent, he appears to me poſſeſſed of a general intelligence beyond his age: and he has diſcovered a genius and inclination for drawing, nearly as ſtrong as for muſic; for whenever he is not at an inſtrument, he uſually employs himſelf in ſketching, with his left-hand, houſes, churches, ſhips, or animals, in his rude and wild manner, with chalk on the floor, or upon whatever other plain ſurface he is allowed to ſcrawl. Painters may, perhaps, form ſome judgment of his muſic by his drawings.

"to hear him, and the muſical people are all amaze-
"ment[(b)]."

The child being but two years and eight months old when this letter was written, his performance muſt have appeared conſiderably more wonderful than at preſent: for as he ſeems to have received ſcarce any inſtructions, and to have purſued no regular courſe of ſtudy or practice ſince that time, it can hardly be imagined that he is much improved. However, experience muſt have informed him what ſeries or combination of ſounds was moſt offenſive to his ear; but ſuch is his impetuoſity that he never dwells long on any note or chord, and indeed his performance muſt originally have been as much under the guidance of the eye as the ear, for when his hand unfortunately falls upon wrong notes, the ear cannot judge till it is too late to correct the miſtake. However, habit, and perhaps the delicacy and acuteneſs of another ſenſe, that of feeling, now direct him to the keys which he preſſes down, as he hardly ever looks at them.

(b) His father, who has lately been in London, and with whom I have converſed ſince this account was drawn up, all the particulars of which he has confirmed, told me, that when he firſt carried the child to the cathedral he uſed to cry the inſtant he heard the loud organ, which, being ſo much more powerful than that to which he had been accuſtomed at home, he was ſome time before he could bear without diſcovering pain, occaſioned, perhaps, by the extreme delicacy of his ear, and irritability of his nerves.

The firſt voluntary he heard with attention was performed at his father's houſe by Mr. MULLY, a muſic-maſter; and as ſoon as he was gone, the child ſeeming to play on the organ in a wild and different manner from what his mother was accuſtomed to hear, ſhe aſked him what he was doing? And he replied, "I am playing "the gentleman's fine thing." But ſhe was unable to judge of the reſemblance: however, when Mr. MULLY returned a few days after, and was aſked, whether the child had remembered any of the paſſages in his voluntary, he anſwered in the affirmative. This happened about the middle of November 1777, when he was only two years and four months old, and for a conſiderable time after he would play nothing elſe but theſe paſſages.

A muſical gentleman of Norwich informed Mr. PARTRIDGE, that, at this time, ſuch was the rapid progreſs he had made in judging of the agreement of ſounds, that he played the Eaſter-Hymn with full harmony; and in the laſt two or three bars of *Hallelujah*, where the ſame ſound is ſuſtained, he played chords with both hands, by which the parts were multiplied to ſix, which he had great difficulty in reaching on account of the ſhortneſs of his fingers. The ſame gentleman obſerved, that in making a baſe to tunes which he had recently caught by his ear, whenever the harmony diſpleaſed him, he would con-

tinue the treble note till he had formed a better accompanyment.

From this period his memory was very accurate in retaining any tune that pleaſed him: and being preſent at a concert where a band of gentlemen-performers played the overture in Rodelinda, he was ſo delighted with the minuet, that the next morning he hummed part of it in bed; and by noon, without any further aſſiſtance, played the whole on the organ.

His chief delight at preſent is in playing voluntaries, which certainly would not be called muſic if performed by one of riper years, being deficient in harmony and meaſure; but they manifeſt ſuch a diſcernment and ſelection of notes as is truly wonderful, and which, if ſpontaneous, would ſurprize at any age. But though he executes fragments of common tunes in very good time, yet no adherence to any particular meaſure is diſcoverable in his voluntaries; nor have I ever obſerved in any of them that he tried to play in triple time. If he diſcovers a partiality for any particular meaſure, it is for dactyls of one long and two ſhort notes, which conſtitute that ſpecies of common time in which many ſtreet-tunes are compoſed, particularly the firſt part of the Belleiſle March, which, perhaps, may firſt have ſuggeſted this meaſure to him, and impreſſed it in his memory. And

his

his ear, though exquiſitely formed for diſcriminating ſounds, is as yet only captivated by vulgar and common melody, and is ſatisfied with very imperfect harmony. I examined his countenance when he firſt heard the voice of Signor PACCHIEROTTI, the principal ſinger of the opera, but did not find that he ſeemed ſenſible of the ſuperior taſte and refinement of that exquiſite performer; however, he called out very ſoon after the air was begun, "He is ſinging in F."

And this is one of the aſtoniſhing properties of his ear, that he can diſtinguiſh at a great diſtance from any inſtrument, and out of ſight of the keys, any note that is ſtruck, whether A, B, C, &c. In this I have repeatedly tried him, and never found him miſtaken even in the half notes; a circumſtance the more extraordinary, as many practitioners and good performers are unable to diſtinguiſh by the ear at the opera or elſewhere in what key any air or piece of muſic is executed.

But this child was able to find any note that was ſtruck in his hearing, when out of ſight of the keys, at two years and a half old, even before he knew the letters of the alphabet: a circumſtance ſo extraordinary, that I was very curious to know when, and in what manner, this faculty firſt diſcovered itſelf; and his father ſays, that in the middle of January 1778, while he was playing the organ, a particular note hung, or, to ſpeak the

language

language of organ-builders, ciphered, by which the tone was continued without the preſſure of the finger: and though neither himſelf nor his elder ſon could find out what note it was, the child, who was then amuſing himſelf with drawing on the floor, left that employment, and going to the organ immediately laid his hand on the note that ciphered[c]. Mr. CROTCH thinking this the effect of chance, the next day purpoſely cauſed ſeveral notes to cipher, one after the other, all which he inſtantly diſcovered: and at laſt he weakened the ſprings of two keys at once, which, by preventing the valves of the wind-cheſt from cloſing, occaſioned a double cipher, both of which he directly found out. Any child, indeed, that is not an idiot, who knows black from white, long from ſhort, and can pronounce the letters of the alphabet by which muſical notes are called, may be taught the names of the keys of the harpſichord in five minutes[d]; but, in general, five years would not be ſufficient, at any age, to impreſs the mind of a muſical ſtudent with an infallible reminiſcence of the tones produced by theſe keys, when not allowed to look at them.

(c) This circumſtance proves that he exerciſed his eye in drawing, after his manner, before he was two years and a half old.

(d) By remarking that the ſhort keys, which ſerve for flats and ſharps, are divided into parcels of threes and twos, and that the long key between every two ſhort keys is always called D, it is extremely eaſy from that note to diſcover the ſituation and names of the reſt, according to the order of the firſt ſeven letters of the alphabet.

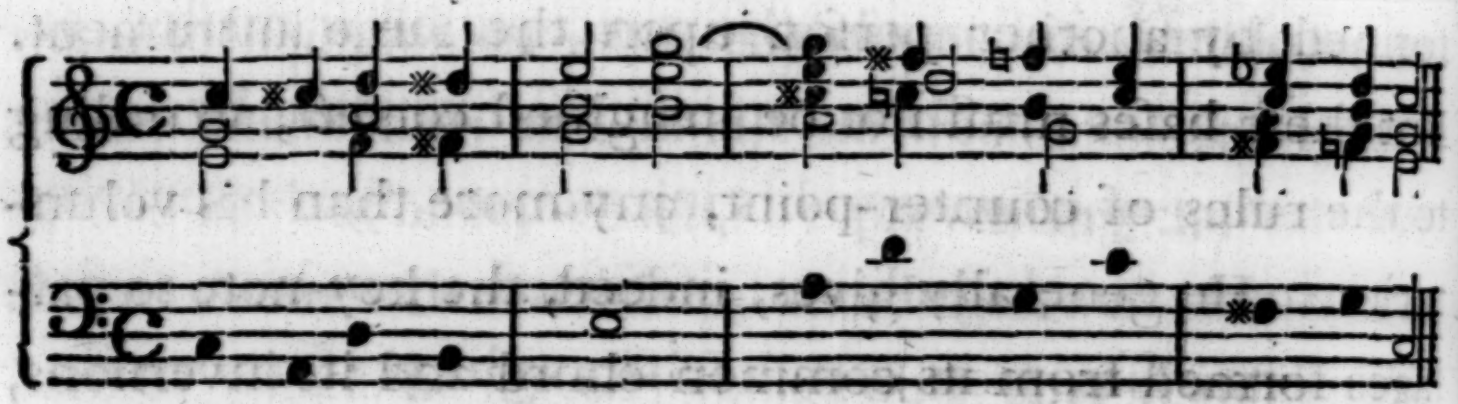
Another

Another wonderful part of his pre-maturity was the being able at two years and four months old to tranſpoſe into the moſt extraneous and difficult keys whatever he played; and now, in his extemporaneous flights, he modulates into all keys with equal facility.

The laſt qualification which I ſhall point out as extraordinary in this infant muſician, is the being able to play an extemporary baſe to eaſy melodies when performed by another perſon upon the ſame inſtrument. But theſe baſes muſt not be imagined correct, according to the rules of counter-point, any more than his voluntaries. He generally gives, indeed, the key-note to paſſages formed from its common chord and its inverſions, and is quick at diſcovering when the fifth of the key will ſerve as a baſe. At other times he makes the third of the key ſerve as an accompaniment to melodies formed from the harmony of the chord to the key-note; and if ſimple paſſages are played ſlow, in a regular progreſſion aſcending or deſcending, he ſoon finds out that thirds or tenths, below the treble, will ſerve his purpoſe in furniſhing an agreeable accompaniment.

However, in this kind of extemporary baſe, if the ſame paſſages are not frequently repeated, the changes of modulation muſt be few and ſlow, or correctneſs cannot be expected even from a profeſſor. The child is always as ready at finding a treble to a baſe as a baſe to a treble,

if

if played in ſlow notes, even in chromatic paſſages; that is, if, after the chord of C natural is ſtruck, C be made ſharp, he ſoon finds out that A makes a good baſe to it; and on the contrary, if, after the chord of D with a ſharp third, F is made natural, and A is changed into B, he inſtantly gives G for the baſe. Indeed he continued to accompany me with great readineſs in the following chromatic modulation, aſcending and deſcending:

I made more experiments of this kind, but to relate them would render my account too technical to all but compoſers, or ſuch as have long ſtudied harmony.

When he declares himſelf tired of playing on an inſtrument, and his muſical faculties ſeem wholly blunted, he can be provoked to attention, even though engaged in any new amuſement, by a wrong note being ſtruck in the melody of any well-known tune; and if he ſtands by the inſtrument when ſuch a note is deſignedly ſtruck, he will inſtantly put down the right, in whatever key the air is playing.

At preſent, all his own melodies are imitations of common and eaſy paſſages, and he ſeems inſenſible to others; however,

however, the only method by which ſuch an infant can as yet be taught any thing better ſeems by example. If he were to hear only good melody and harmony, he would doubtleſs try to produce ſomething ſimilar; but, at preſent, he plays nothing correctly, and his voluntaries are little leſs wild than the native notes of a lark or a black-bird. Nor does he, as yet, ſeem a ſubject for inſtruction: for till his reaſon is ſufficiently matured to comprehend and retain the precepts of a maſter, and ſomething like a wiſh for information appears, by a ready and willing obedience to his injunctions, the trammels of rule would but diſguſt, and, if forced upon him, deſtroy the miraculous parts of his ſelf-taught performance.

Mr. BAILLET publiſhed in the laſt century a book, *Sur les Enfans celebres par leurs etudes*; and yet, notwithſtanding the title of his work, he ſpeaks not of infants but adoleſcents, for the youngeſt wonder he celebrates in literature is at leaſt ſeven years old; an age at which ſeveral ſtudents in muſic under my own eye have been able to perform difficult compoſitions on the harpſichord, with great neatneſs and preciſion. However, this has never been accompliſhed without inſtructions and laborious practice, not always voluntary.

Muſical prodigies of this kind are not infrequent: there have been ſeveral in my own memory on the harpſichord. About thirty years ago I heard PALSCHAU, a

German boy of nine or ten years old, then in London, perform with great accuracy many of the moſt difficult compoſitions that have ever been written for keyed inſtruments, particularly ſome leſſons and double fugues by SEBASTIAN BACH, the father of the preſent eminent profeſſors of that name, which, at that time, there were very few maſters in Europe able to execute, as they contained difficulties of a particular kind; ſuch as rapid diviſions for each hand in a ſeries of thirds, and in ſixths, aſcending and deſcending, beſides thoſe of full harmony and contrivance in nearly as many parts as fingers, ſuch as abound in the leſſons and organ fugues of HANDEL.

Miſs FREDERICA, now Mrs. WYNNE, a little after this time, was remarkable for executing, at ſix years old, a great number of leſſons by SCARLATTI, PARADIES, and others, with the utmoſt preciſion.

But the two ſons of the reverend Mr. WESTLEY ſeem to have diſcovered, during early infancy, very uncommon faculties for the practice of muſic. CHARLES, the eldeſt, at two years and three quarters old, ſurprized his father by playing a tune on the harpſichord readily, and in juſt time: ſoon after he played ſeveral, whatever his mother ſung, or whatever he heard in the ſtreet.

SAMUEL, the youngeſt, though he was three years old before he aimed at a tune, yet by conſtantly hearing his brother practiſe, and being accuſtomed to good muſic

and maſterly execution, before he was ſix years old arrived at ſuch knowledge in muſic, that his extemporary performance on keyed inſtruments, like MOZART's, was ſo maſterly in point of invention, modulation, and accuracy of execution, as to ſurpaſs, in many particulars, the attainments of moſt profeſſors at any period of their lives.

Indeed MOZART, when little more than four years old, is ſaid to have been "not only capable of executing leſ-"ſons on his favourite inſtrument, the harpſichord, but "to have compoſed ſome in an eaſy ſtyle and taſte, which "were much approved[e]:" and SAMUEL WESTLEY before he could write was a compoſer, and mentally ſet the airs of ſeveral oratorios, which he retained in memory till he was eight years old, and then wrote them down.

Here the difference of education appears: little CROTCH, left to nature, has not only been without inſtructions but good models of imitation; while MOZART and SAMUEL WESTLEY, on the contrary, may be ſaid to have been nurſed in good muſic: for as the latter had his brother's excellent performance to ſtimulate attention, and feed his ear with harmony; the German infant, living in the houſe of his father, an eminent profeſſor,

(e) See Phil. Tranſ. vol. LX. for the year 1770; an account of a very remarkable young muſician, by the honourable DAINES BARRINGTON, F. R. S. who ſoon intends to favour the public with an account of the two WESTLEYS.

and an elder ſiſter, a neat player on the harpſichord, and conſtantly practiſing compoſitions of the firſt claſs for that inſtrument, had every advantage of ſituation and culture joined to the profuſion of natural endowments.

Of MOZART'S infant attempts at muſic I was unable to diſcover the traces from the converſation of his father; who, though an intelligent man, whoſe education and knowledge of the world did not ſeem confined to muſic, confeſſed himſelf unable to deſcribe the progreſſive improvements of his ſon during the firſt ſtages of infancy. However, at eight years of age I was frequently convinced of his great knowledge in compoſition by his writings; and that his invention, taſte, modulation, and execution in extemporary playing, were ſuch as few profeſſors are poſſeſſed of at forty years of age.

Into what the preſent prodigy may mature is not eaſy to predict; we more frequently hear of trees in bloſſom during the winter months than of fruits in conſequence of ſuch unſeaſonable appearances. However, to keep pace with the expectations to which ſuch premature talents give birth is hardly allowed to humanity. It is the wiſh of ſome, that the uncommon faculties with which this child is endowed might be ſuffered to expand by their own efforts, neither reſtrained by rules, nor guided by examples; that, at length, the world might be furniſhed with a ſpecies of natural muſic, ſuperior to all the ſurprizing

productions

productions of art to which pedantry, affectation, or a powerful hand, have given birth. But, alas! ſuch a wiſh muſt have been formed without reflexion; for muſic having its claſſics as well as poetry and other arts, what could he compoſe or play upon different principles that would not offend the ears of thoſe who have regarded thoſe claſſics as legiſlators, and whoſe ſouls have been wrapped in elyſium by their ſtrains? He might as well, if ſecluded from all intercourſe with men, be expected to invent a better language than the preſent Engliſh, the work of millions, during many centuries, as a new muſic more grateful to the ears of a civilized people than that with which all Europe is now delighted.

An individual may doubtleſs advance nearer perfection in every art by the aſſiſtance of thouſands, than by the mere efforts of his own labour and genius.

Another wiſh has been formed, that the effects of different genera and diviſions of the muſical ſcale might be tried upon this little muſician; but the ſucceſs of ſuch an experiment is not difficult to divine. An uncultivated ear would as naturally like the moſt plain and com mon muſic, as a young mind would beſt comprehend the moſt ſimple and evident propoſitions: and, as yet, the attention of CROTCH cannot be excited by any muſical refinements or elaborate contrivance.

It has likewiſe been imagined by ſome, that every child might be taught muſic in the cradle, if the experiment were made; but to theſe it may with truth be ſaid, that ſuch an experiment is *daily* made on every child, by every mother and nurſe, that is able to form a tune, on every part of the globe. In Italy the *ninne nonne*, or lullabies, are fragments of elegant melodies, become common and popular by frequent hearing; and theſe, though they help to form the national taſte, are not found to ſtimulate the attention of Italian children to melody, or to accelerate the diſplay of muſical talents at a more early period than elſewhere.

Premature powers in muſic have as often ſurprized by ſuddenly becoming ſtationary as by advancing rapidly to the ſummit of excellence. Sometimes, perhaps, nature is exhauſted or enfeebled by theſe early efforts; but when that is not the caſe, the energy and vigour of her operations are ſeldom properly ſeconded, being either impeded and checked by early ſelf-complacence, or an injudicious courſe of ſtudy; and ſometimes, perhaps, genius is kept from expanſion by ill-choſen models; excluſive admiration, want of counſel, or acceſs to the moſt excellent compoſitions and performers in the claſs for which nature has fitted thoſe on whom it is beſtowed.

Fig. 1

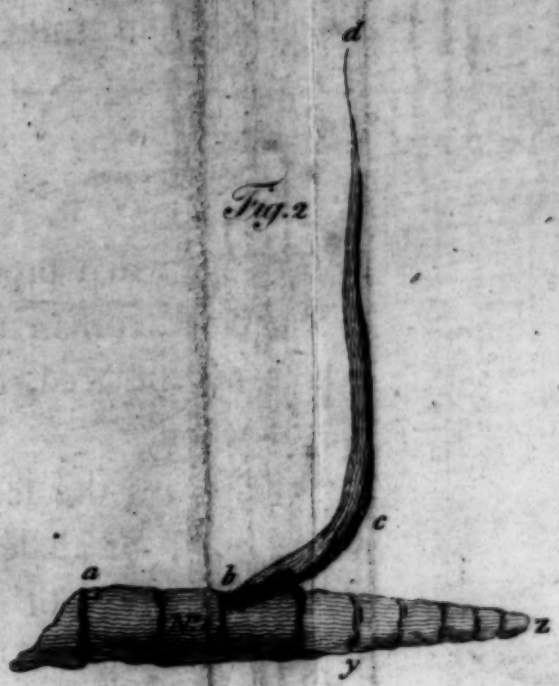


Fig. 2

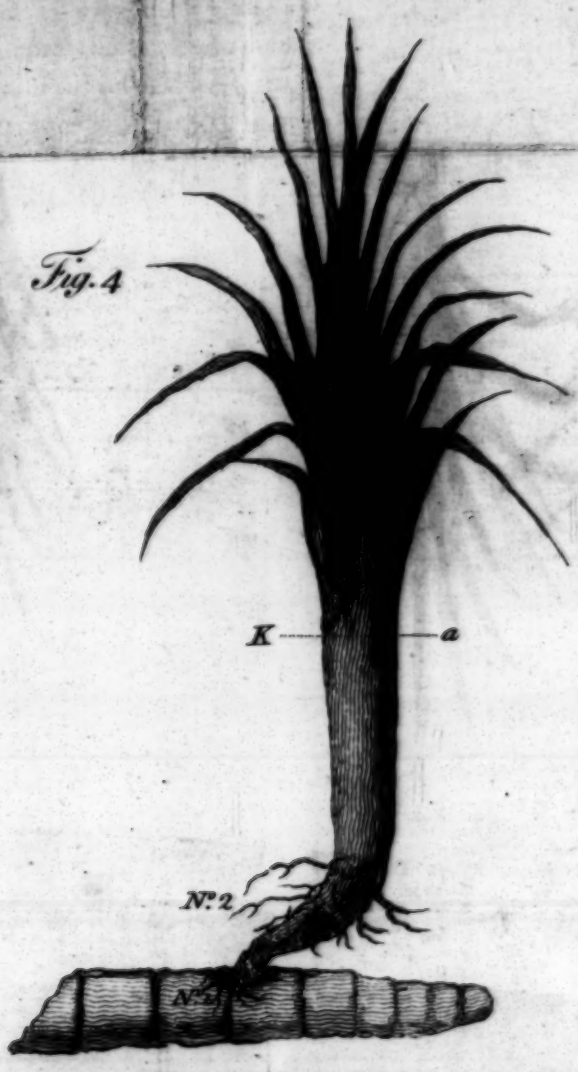
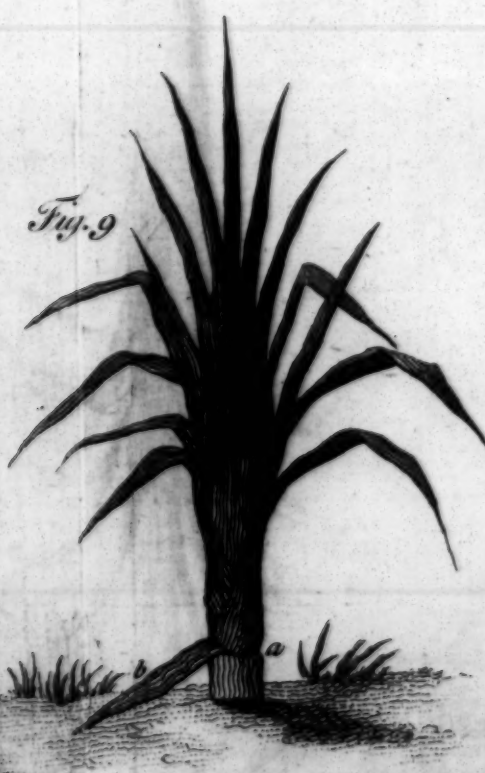


Fig. 4

Fig. 7

Fig. 8.



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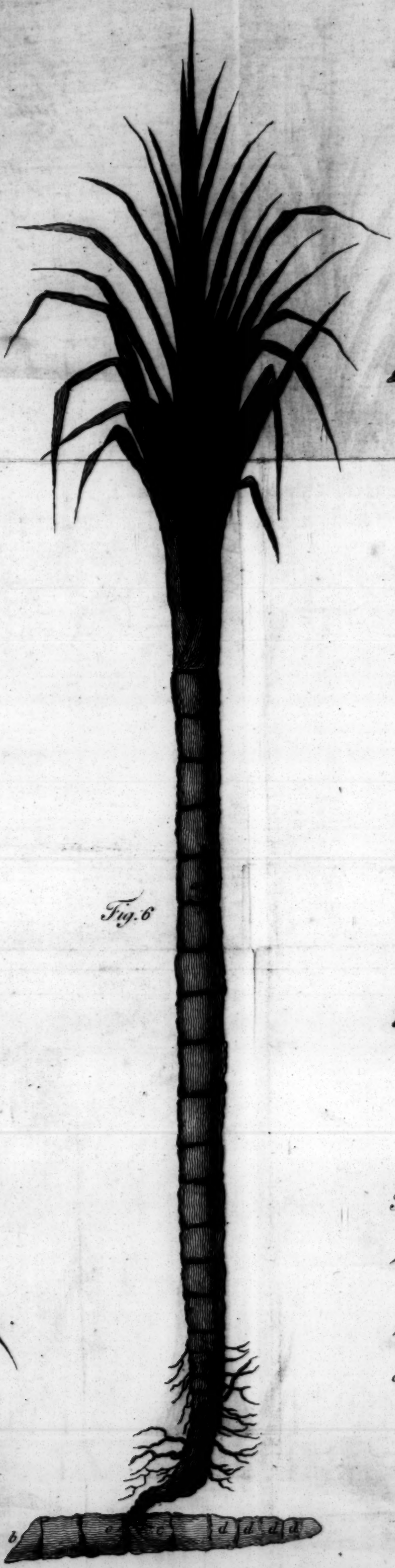


Fig. 6

Fig. 3

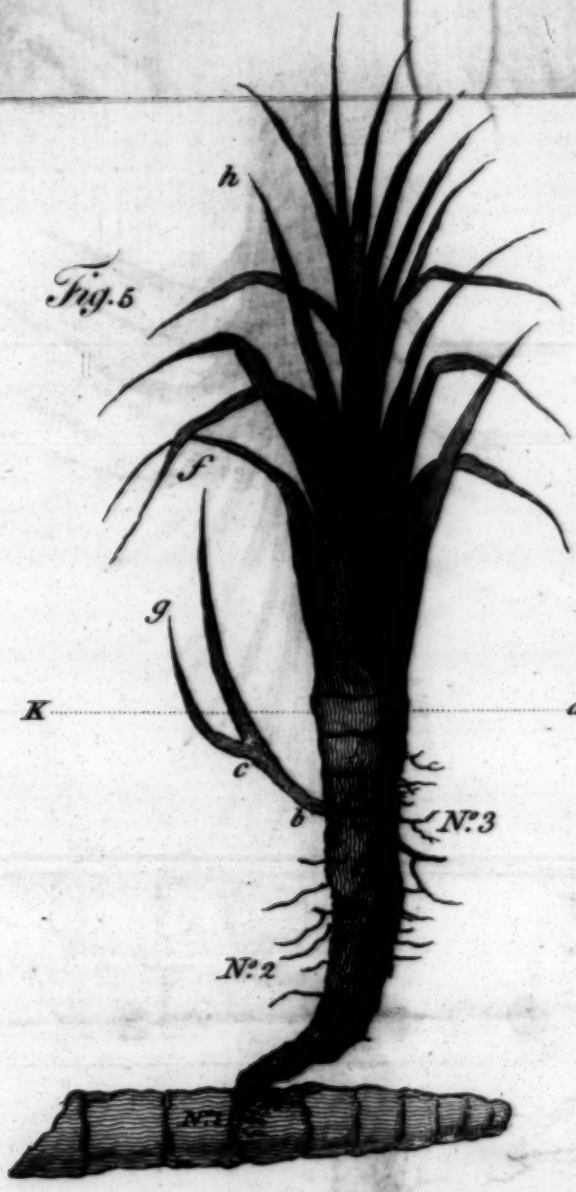


Fig. 5

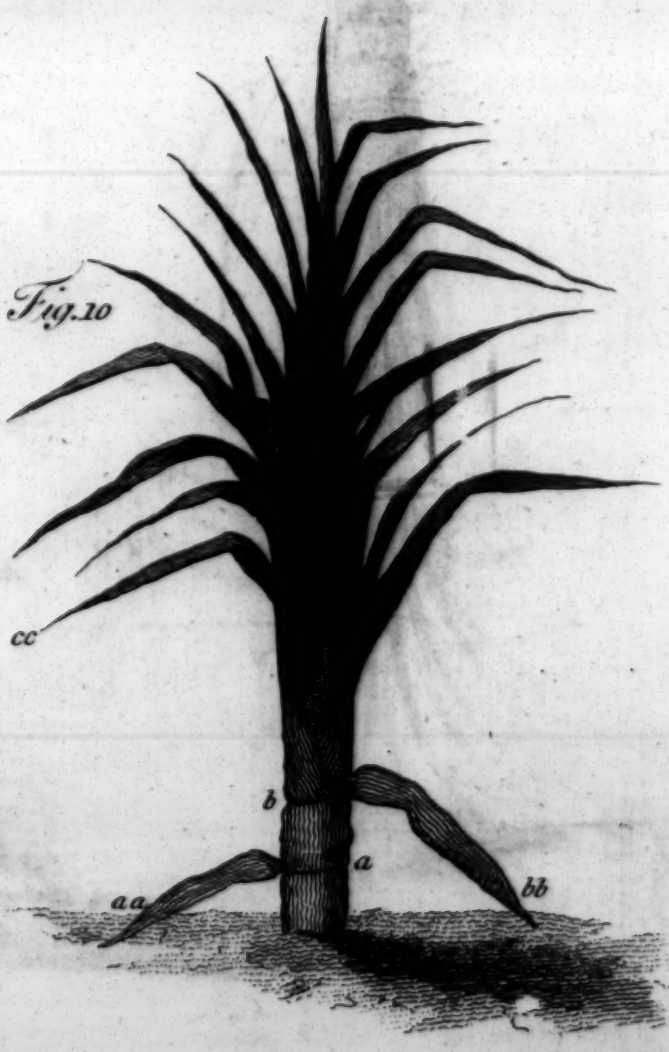
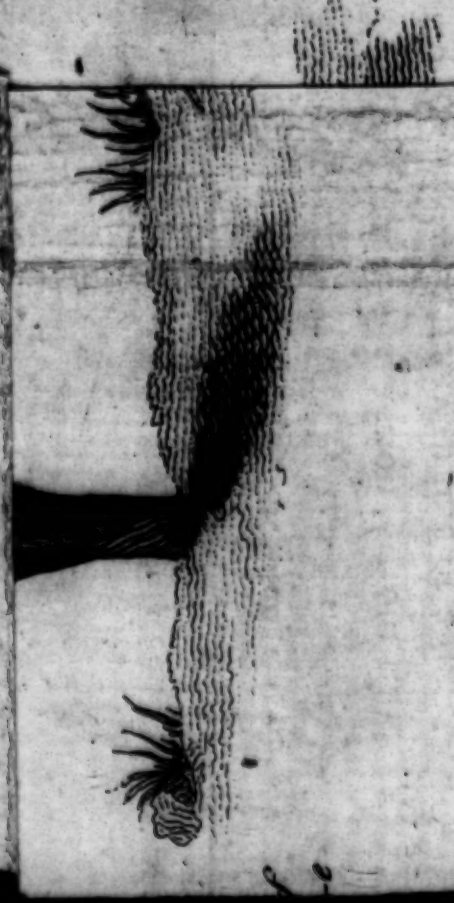


Fig. 10

Fig. 2



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XIX. *Account of a new Method of cultivating the Sugar Cane. By Mr.* Cazaud; *communicated by* Joſeph Banks, *Eſq. P. R. S.*

Read February 25, 1779.

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Vue

THOUGH the work which I have the honour to lay before the Royal Society was undertaken entirely with a view to private advantage, it appeared to me not entirely deſtitute of public utility, were that utility no other than what may ariſe from there exiſting a regular hiſtory of a plant which is worth nine millions ſterling to Europe.

The uſual method of cultivating the ſugar cane appears more like the conſequence of ſome general obſervations than the reſult of ſuch as ought to have been purpoſely made.

The methods in uſe, before mine, are reducible to two.

The

L'OUVRAGE que j'ay l'honneur de preſenter et de ſoumettre au jugement de la Societé Royale de Londres, entrepris uniquement pour des vues particulieres, m'a paru ſuſceptible d'une utilité générale, quand il ne devroit produire qu'une diſcuſſion ſur une plante qui fournit à l'Europe une reproduction annuelle de neuf millions ſterling.

La culture ordinaire de la canne me paroit plutôt être le fruit de quelques obſervations générales que le reſultat de celles qu'on auroit du faire.

On peut reduire à deux methodes, les ſyſtemes établis avant celuy que je propoſe.

The one conſiſts in making ſugar in almoſt all ſeaſons indifferently, conſequently in planting rather (if one may ſo ſay) when the planter is beſt prepared for his work, than at the time which is beſt ſuited to the eſſential end, which undoubtedly ſhould be, *to get from the earth, all that can be expected from its fruitfulneſs after due allowance has been made for the different circumſtances of the growth of the cane, and the revolutions it undergoes in the different ſeaſons.*

This firſt method aroſe, from the want of proper obſervations, and the difficulty of procuring the number of negroes requiſite to work in another manner; ſo that the deſire of cultivating a greater quantity of land always having kept pace with the augmentation

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L'une conſiſte à faire du ſucre preſque indiſtinctement en tout tems, et conſéquemment à planter, s'il eſt permis de le dire, plutôt dans le tems où l'on eſt le mieux arrangé pour cette operation, que dans celuy qui conviendroit le plus au but eſſentiel, qui doit être, *de tirer de la terre tout ce qu'on peut eſpérer de ſa fertilité, combinée avec les différentes circonſtances de l'accroiſſement de la canne, et des revolutions qu'elle eprouve dans les différentes ſaiſons.*

Cette methode a été une ſuite neceſſaire de la difficulté qu'on avoit dans le principe de ſe procurer la quantité de negres qu'il auroit fallu pour travailler autrement, jointe au manque d'obſervations qui puſſent encore diriger. Le deſir de cultiver une plus grande quantité de terre augmentant avec le nombre des negres,

tation of the negroes, the latter muſt of courſe have been always inadequate to the work; whence the only change ſuch a method of culture could ever admit of conſiſted in making a little more ſugar in a good ſeaſon, and a little leſs in a bad one.

The ſecond method, which I call the ſyſtem of cultivation in the great way, and is that which has been either originally followed or adopted wherever the number of negroes has been anſwerable to the aſſurance the ſlave-merchant had of receiving the price he had agreed to take for them, conſiſts, firſt, in planting a fourth or fifth of one's land in October, November, and December, when, all the other buſineſs of the plantation being finiſhed, there is time to give to this important one all the attention it deſerves.

2dly,

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negres, dont la quantité ſe trouvoit toujours ainſi diſproportionée aux travaux, le ſeul changement que l'avidité d'etendre ſes plantations ait pu admettre dans le premier plan, a donc été de faire un peu plus de ſucre dans la bonne ſaiſon et un peu moins dans la mauvaiſe.

L'autre methode que j'appelle le ſyſteme de la grande culture a été ſuivie où adoptée dans tous les païs où la facilité de ſe procurer des negres a été proportionnée à l'avantage qu'on avoit ſoin d'aſſurer au marchand, d'en recevoir le payement aux termes ſtipulés. Cette methode conſiſte, 1°, à planter le quart où le cinquieme de ſa terre en Octobre, Novembre, et Decembre, parce qu' alors tous les autres travaux etant finis, on eſt tout entier à cette opération importante.

2dly, In digging very deep trenches, becauſe the deeper the trench the greater the nouriſhment of the root.

3dly, In planting at great diſtances, by which the air circulates more freely between the plants, and by that means enſures them a quicker and more compleat maturity.

Finally, in cutting the canes in the four fineſt months of the year, to wit, in February, March, April, and May, becauſe the ſugar is then the fineſt, is cut with the leaſt trouble, and (at leaſt as is pretended) is ſupplied in greater quantities by the canes.

Where this method is followed, about three fourths of the plantation is cut, the remainder is made up partly of young canes, which can only be cut the following year, and partly of ſuch as muſt neceſſarily be ſacrificed and reſerved for the purpoſe of getting

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2°, à faire des foſſes très profondes, parce que les racines trouvent plus de nourriture dans une plus grande profondeur.

3°, à planter à de grandes diſtances, parce que l'air circule mieux entre les plantes, et leur procure une maturité plus prompte et plus parfaite.

4°, enfin, à faire la recolte pendant les 4 mois de la plus belle ſaiſon, Fevrier, Mars, Avril, May, parce qu' alors le ſucre ſe fait plus aiſément, plus beau, et que les cannes, dit-on, en donnent une plus grande quantité.

On coupe dans ce ſyſteme environ les trois quarts de la terre deſtinée aux cannes; le reſte eſt partie en jeunes plantes qu'on ne peut couper que l'année d'après, et partie dans quelque portion de terre ſacrifiée neceſſairement pour ſe procurer

getting the plants which are wanted after the crop is over.

My own method, which I now proceed to explain, is this: I employ the whole of the ſix firſt months of the year in the buſineſs of the crop, and in May and June plant the canes which have been cut in January. This of courſe induces a neceſſity of cutting the rattoons at the end of the eleventh inſtead of the end of the twelfth month, and the planted canes, which ſhould ſtand fifteen months, at the end of the year: in return, the whole plantation is cut every year.

It is objected, that beſides the firſt loſs from cutting the rattoons one, and the planted canes three months before their time, there is a ſecond which ariſes from what is cut not being ſufficiently ripe. Theſe objections I mean to anſwer by the hiſtory of the cane, that of the ſeaſons, that

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procurer le plan dont on a beſoin dans une ſaiſon où la recolte eſt déja faite.

Je propoſe d'employer en entier les ſix premiers mois de l'année à faire la recolte, et de planter en May et Juin, les cannes coupées en Janvier, ce qui entraine la neceſſité de couper toujours, les rejettons à onze mois au lieu de les couper à douze, et les cannes plantées à un au lieu de les couper à 15 mois: mais on coupe chaque année toute la terre deſtinée aux cannes.

On objecte à ce ſyſteme, oùtre la perte qui reſulte de l'anticipation d'un mois de coupe pour les rejettons, et de trois mois pour les cannes plantées, le defaut de maturité qui ſuit de cette anticipation; je réponds à ces objections par l'hiſtoire de

that of the effect of thoſe ſeaſons, and from experience.

After this, as it is an advantageous conſequence (though not a fundamental principle) of my method, that I only plant a ſixth part of my land every year, and it is objected to this, that there are ſome ſoils which will not give ſo much as a *ſecond* crop, I endeavour to inveſtigate what cauſes can deprive a plant propagating by the gem of its power of giving ſhoots *ad infinitum*, and I find none but ſuch ariſe from a bad method of cultivation.

Having written in the beginning only with a view of inſtructing my overſeer, and by no means with the preſumption of adding to the treaſure of natural hiſtory by the account of a plant which deſerves the pen of a BUFFON, my obſervations (though I flatter myſelf extended to every

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de la canne, des ſaiſons, de leurs effets, et de l'expérience.

Je ne plante auſſi annuellement que le ſixieme de ma terre, mais ce n'eſt pas l'article fondamental du ſyſteme, il me paroît ſeulement en être une ſuite avantageuſe: cependant comme on objecte contre cet article, l'impoſſibilité prétendue, même d'une ſeconde coupe dans certaines terres, je cherche les raiſons qui peuvent annéantir, dans une plante vivace par ſa racine, la faculté de rejettonner juſqu'a la conſommation des ſiecles; et je ne puis les trouver que dans un mauvais ſyſtême de culture.

Quoique je n'aye ecrit dans le principe que pour inſtruire mon œconome, et point du tout avec la préſomption d'enrichir le depot d'hiſtoire naturelle, de celle d'une plante digne de la plume d'un BUFFON, je ne crois pas cependant qu'il y ait un ſeul point intereſſant de cette hiſtoire qui m'ait echappé, mais tous etoient

every material point) were ſcattered, confuſed and drawn up in the natural order they offered themſelves to me, when the neceſſity of diſcuſſing or defending any part of my method obliged me to ſtudy the nature of the plant, to try to find out the reaſon of ſome phenomena which the experience of many years had taught me the exiſtence of; but I have been obliged to follow a different method now that theſe obſervations are to be ſubmitted to the natural judges of every thing that can have influence upon the progreſs of uſeful knowledge. In order to form a proper judgment of the merit of the mode of culture I propoſe, it is neceſſary to be acquainted with the plant and the climate in which it grows.

Obſer-

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etoient epars, ſans ordre, comme ils s'etoient preſentés, à meſure qu'il falloit diſcuter et etablir quelque partie de mon ſyſteme, et que j'étois obligé de chercher dans la nature de la plante, la raiſon de quelque phenomene que pluſieurs années d'expérience m'avoient fait obſerver. Mon deſſein en les rapprochant, etant de les preſenter aux juges naturels de tout ce qui peut influer ſur le progrès des connoiſſances utiles, j'ay du ſuivre une autre methode: pour apprécier la culture que je propoſe il faut connoître la plante, et le climat où on la cultive.

Obſervations on the climate.

In the Windward Iſlands, the weather is commonly dry from the 15th of February to the 15th of May. The rains are moderate till Auguſt; they are very copious the two or three following months, and afterwards decreaſe till February; conſequently there is a ſucceſſion of nine months rain, and of three months dry weather. The annexed table ſhews the quantity of rain fallen at Grenada in the Eaſt quarter from the firſt of June 1772 to the firſt of June 1773, and this is the rain of a common year.

Table

Obſervations ſur le climat.

Dans les Iſles Antilles, du 15 Fevrier au 15 May, il fait ordinairement ſec, les pluyes ſont moderées juſqu'en Aouſt, elles ſont tres fortes pendant les deux où trois mois ſuivants, elles diminuent enſuite juſqu'en Fevrier: voila donc une ſucceſſion de neuf mois de pluye ſuivie de trois mois de ſec; voici le tableau de la pluye tombée à la Grenade dans la partie de l'eſt depuis le 1 Juin 1772 juſqu'au 1 Juin 1773; c'eſt le tableau d'une année commune.

Tableau

Table of rain fallen in the Eaſt quarter of Grenada *from the* 1*ſt of* June 1772 *to the* 1*ſt of June* 1773.

	In.	Tenths of an inch Eng. meaſure.
June -	9	3
July -	13	9
Auguſt -	11	9
September -	19	0
October -	12	7
November -	9	1
December -	18	8
January -	9	5
February -	6	4
March -	2	6
April -	0	8
May -	2	9
Total of inches of rain,	116	0

The

Tableau de la pluye tombée à la Grenade *dans la partie de l'eſt depuis le* 1 Juin 1772 *juſqu'au* 1 Juin 1773.

	Pou.	10e de pouce Anglois.
Juin -	9	3
Juillet -	13	9
Aouſt -	11	9
Septembre -	19	0
Octobre -	12	7
Novembre -	9	1
Decembre -	18	8
Janvier -	9	5
Fevrier -	6	4
Mars -	2	6
Avril -	0	8
May -	2	9
Total des pouces d'eau	116	0

The natural hiſtory of the cane.

There are but few of the minute particulars I am going to mention, but what are ſome how or other connected with my method.

The upper part of the cane, commonly called the head, is the beſt plant that can be uſed to propagate it, ſee fig. 7. ſee likewiſe fig. 1.; it is the part from *l* to *n*, which in this figure is without its leaves. It is a known fact, that the body of the cane does not come up well, unleſs there are continued rains from the time of its being put into the ground, till all the ſhoots are out and pretty ſtrong.

Hiſtory

Hiſtoire naturelle de la canne.

Il eſt peu des details minutieux dans leſquels je vais entrer qui n'ait un rapport quelquonque avec mon ſyſteme.

La partie ſuperieure de la canne appellée communement la tête, eſt le meilleur plan dont on puiſſe ſe ſervir pour la multiplier, v. fig. 7. v. auſſi fig. 1. c'eſt la partie depuis *l* juſqu'a *n*, elle eſt dans cette figure depouillée de ſes feuilles: il eſt reconnu que le corps de la canne ne reuſſit point à moins d'une pluye continuelle depuis le moment qu'il eſt en terre, juſqu'au moment où tous les jets ſont ſortis et aſſez vigoureux.

Hiſtoire

Hiſtory of the roots of the ſugar cane, and of its productions under ground.

If the plant be put into the ground as ſoon as it is cut, and the weather happens to be very hot and dry, a fortnight often paſſes before the eye can diſcover any alteration in the two or three moſt promiſing joints; the firſt being too hard (ſee *a* fig. 2.) become immediately dry; the others (ſee from *y* to *z* in the ſame figure) are only graſs, and produce nothing, unleſs when the laſt happens to ſhoot at its extremity.

When the dry weather continues, but ſo as not materially to affect the ſoil in contact with the inferior part of the plant, one diſcovers at the end of three weeks, about that part of the joint which lies undermoſt in the trench, ſome

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Hiſtoire des racines de la canne et de ſes productions en terre.

1°, Si le plan eſt mis en terre auſſi tôt qu'il eſt coupé, et qu'il ſurvienne de grandes chaleurs ſans pluye, il eſt ſouvent quinze jours ſans que l'œil decouvre la moindre alteration dans les deux ou trois nœuds de la plus grande eſpérance; les premiers, trop durs, v. *a*, fig. 2. ſont d'abord deſſechês; les autres, herbacés, v. *y* juſqu'a *z*, ne produiſent rien à moins que le dernier ne ſe developpe par ſon extremité.

Si le ſec continue et qu'il ne ſe faſſe pas ſentir vivement juſqu'à la terre qui touche la partie inferieure du plan, ou decouvre après trois ſemaines à cette partie

ſome white fibres of about half a line in length and thickneſs: notwithſtanding, however, this beginning of vegetation, the plant ſtill dies, unleſs it rains, before the ſoil in contact with theſe ſmall roots be entirely dry; at leaſt I have always found the earth dry under theſe ſmall roots, when, in conſequence of a long drought, the plant from which they had come was entirely dried up. But I have ſeen plants, put into the ground as ſoon as cut, put forth one, and ſometimes two ſprigs at the end of ſix weeks, though they had been deprived of rain for above a month. When the plant is ſomewhat withered, that is, when it has not been planted till ſome days after having been cut, it thrives ſooner when aſſiſted by rain, and dies ſooner when deprived of it: the reaſon I take to be this, it could not wither without loſing ſome part of that moiſture which it contains, and which it wanted

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du nœud qui touche le fond de la foſſe, quelques fils blancs de la longueur et de la groſſeur d'une demi ligne: après ce commencement de végetation, le plan meurt s'il ne pleut pas avant que la terre qui touche ces petites racines ſoit entiérement deſſechée, ou du moins j'ay toujours trouvé la terre ſeche ſous ces petites racines lorſqu' après un long ſec, j'ay trouvé le plan qui les avoit données entiérement deſſeché; mais j'ay vu du plan mis en terre auſſi-tôt que coupé privé de pluye pendant plus d'un mois donner un et quelquefois deux jets après ſix ſemaines: ſi le plan eſt un peu fanné, c'eſt à dire coupé quelques jours avant d'être mis en terre, il leve plus vite s'il eſt ſecondé de la pluye, et meurt plutôt s'il en eſt privé, parce qu'il n'a pu ſe fanner ſans perdre une partie de l'eau qu'il contenoit, dont

wanted to preſerve it from the dryneſs of the ſurrounding earth, which earth itſelf alſo probably ſucks up part of its humidity: *I am, therefore, apt to think, though contrary to the opinion of a great many people, that it is beſt to put the plant into the ground as ſoon as it is cut.*

2dly, We all along ſuppoſe the weather to be tolerably favourable; then, when the ſmall roots, which ſprout about the joint when the ſurrounding earth has its proper degree of moiſture, are of the length of a line and a half or two lines, the bud adherent to it ſwells; a few days after it lengthens horizontally in the direction of the plant, ſee *b*, fig. 2.; it next deſcribes a ſpiral, *c*; and at length comes out of the earth like the point of an arrow, *d*. Theſe are what we call the firſt productions; they are the immediate productions of the plant. The ſhaft

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il auroit eu beſoin pour ſe ſoutenir plus longtems contre la ſechereſſe de la terre qui l'environne, et qui probablement pompe même une partie de ſon humidité: *je crois donc contre l'opinion de beaucoup de gens qu'il vaut mieux mettre le plan en terre auſſi-tôt qu'il eſt coupé.*

2°, Nous ſuppoſerons un tems aſſez favorable; lorſque ces petites racines qui ſe forment à l'entour du nœud ſi la terre eſt par tout humide, ſont longues d'une ligne et demie ou deux, le bouton qui luy eſt attaché paroit gonflé, quelques jours après il s'allonge horizontalement, v. *b*, fig. 2. ſuivant la direction dans la quelle le plan eſt couché, enſuite il decrit une ſpirale, v. *c*, même figure, et ſort de terre comme une pointe de fleche *d*. C'eſt ce que j'appelle les premiéres productions,

ſhaft of the arrow then breaks, the two firſt leaves expand, ſee *e*, *e*, fig. 3. and riſe from twenty-four to thirty inches high. Theſe are, properly ſpeaking, the leaves of the plant, I ſhall call them the *external* leaves, though they are in fact incloſed within a quadruple ſheath of ſeminal leaves, ſee *f*, *f*, *f*, *f*; but theſe laſt riſe but a very little way out of the ground. I believe, that the joint, to which the laſt leaf of this external caſe is faſtened, ought to be conſidered as the beginning of the ſtem of the plant which divides its lower part (that which produces nothing above ground) from its upper part, which produces things of various kinds; at leaſt, I have never ſeen any thing produced by this lower part, except theſe four ſmall leaves, and ſome roots.

3dly,

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tions, qui ſont les productions directes du plan, Deux ou trois jours après, le faiſceau qui compoſe cette fleche ſe developpe, les deux premieres feuilles ſe deployent, v. *ee*, fig. 3. elles s'elevent juſqu'à 24 et 30 pouces, ce ſont les feuilles proprement dites de la plante; je les appellerai *exterieures* quoiqu'elles ſoient dans leur origine enveloppées dans un quadruple etui de feuilles ſeminales, v. *f*, *f*, *f*, *f*, même figure, parce que celles cy ne s'elevent que très peu au deſſus de la terre: je crois que le nœud auquel eſt attaché la derniere feuille de cette enveloppe doit être regardé comme le collet de la plante qui diviſe ſa partie inferieure qui ne donne point de productions hors de terre d'avec ſa partie ſuperieure, d'où il en ſort de toute eſpéce: je n'ay du moins jamais remarqué dans cette partie inferieure, d'autre production que ces 4 petites feuilles et des racines.

3°,

3dly, Till the external leaves are eight or ten inches high, all the nouriſhment which the plant receives from the earth is by the roots which have ſprouted about the joint to which the bud from whence it was produced was faſtened: theſe may, therefore, be conſidered as *a ſet of roots of the firſt order*, ſee fig. 5. n. 1.

4thly, But when that time comes, the ſeminal leaves which formed the ſheath, wither, grow red, ſoon after dry up, and are finally driven away by a *ſet of roots of the ſecond order*, which appear in the places to which each of theſe ſeminal leaves were faſtened. The fourth figure ſhews the young plant deprived of its four ſeminal leaves, and provided with its ſet of roots of the ſecond order, ſee N° 2, which have taken their place.

5thly,

3°, Juſqu'au moment où les feuilles exterieures ſont hautes de 8 à dix pouces, la terre ne tranſmet de nourriture à la plante que par les racines ſorties à l'entour du nœud auquel etoit attaché le bouton qui l'a produite, et qu'on peut regarder comme un *premier ordre de racines*, v. F. 5. n. 1.

4°, Mais alors les feuilles ſeminales qui compoſoient l'enveloppe, ſe fannent, rougiſſent, peu de tems aprés ſe deſſechent, et ſont bientôt chaſſées *par un ſecond ordre de racines* qui paroiſſent à l'endroit où chacune des feuilles ſeminales etoit attachée, la fig. 4. preſente la jeune plante denuée de ſes quatre feuilles ſeminales et pourvüe de ſon ſecond ordre de racines, v. N° 2. qui a pris leur place.

5thly, Each of the *external* leaves mentioned in the ſecond article (I mean thoſe which appeared firſt) are likewiſe ſo many indications of joints in the ground; ſo much the more diſtinct, the more vigorous the leaf is; and in proportion as each of theſe leaves dries, each of the joints ſucceſſively produces a *row* of roots which conſtitute the *third ſet*, ſee N° 3. fig. 5.

It is to each of theſe rows of roots that the joint is faſtened which is to furniſh the ſuckers which I call the ſecond productions. The fifth figure ſhews the plant without either its ſeminal or its external leaves, the joints of which are in the ground. It appears provided with its firſt, ſecond, and third ſet of roots, N° 1, 2, 3. Each row of this third ſet of roots is intended either to nouriſh the plant which is to come out of the joint which is faſtened

to

5°, Chacune des feuilles *extérieures* mentionnées à l'article 2 (j'entends les premiéres ſorties) annonce auſſi un nœud en terre d'autant plus diſtinct que la feuille eſt plus vigoureuſe, et à meſure que chacune de les feuilles ſe deſſeche, chaque nœud donne ſucceſſivement un *plan* de racines qui forment le *troiſieme ordre*, v. N° 3. fig. 5.

C'eſt à chacun de ces plans de racines qu'eſt attaché le nœud qui doit donner les jets que j'appelle ſecondes productions. La figure 5 preſente la plante denuée tant de ſes feuilles ſeminales que de ſes feuilles exterieures dont les nœuds ſont en terre; elle paroît auſſi pourvue de ſon premier ordre de racines N° 1, de ſon ſecond ordre de racines N° 2, et du 3e N° 3, chaque plan de ce 3e ordre de racines eſt deſtiné à nourrir la plante qui ſortira du bouton qui luy eſt attaché, ſi

la

to it, if the ſeaſon be favourable enough, or if either the ſeaſon or cultivation be unfavourable to the birth of this bud; in that caſe, the roots are to contribute to the nouriſhment of the plant already formed.

6thly, Let us now go back a little. Suppoſe the ſeaſon always to continue favourable, and the firſt ſhoot to be out of the earth a fortnight or three weeks after planting, conſequently the *firſt ſet* of roots formed.

At five or ſix weeks end the ſeminal leaves are expelled by a *ſecond ſet* of roots. In nine or ten weeks the firſt of the *external* leaves begins to give way to the firſt row of the third ſet of roots.

About the twelfth or fourteenth week the bud which adheres to the joint which forms the firſt row of the third ſet of roots ſwells, opens, and likewiſe appears like the ſhaft

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la ſaiſon eſt aſſez favorable, ou bien à nourrir ſeulement la plante déja formée, ſi le défaut de ſaiſon ou de culture s'oppoſe au developpement du bouton.

6°, Revenons ſur nos pas, ſuppoſant toujours la ſaiſon favorable, et le premier jet ſorti de terre après quinze jours ou trois ſemaines, conſéquemment le *premier ordre* de racines déja formé.

A 5 ou 6 ſemaines les feuilles ſeminales cedent au *ſecond ordre* de racines. A 9 ou 10 ſemaines la premiére des feuilles *exterieures* commence à ceder au premier plan des racines du *troiſieme ordre.*

A 12 ou 14 ſemaines le bouton attaché au nœud qui forme ce premier plan de racines du 3e ordre, ſe gonfle, ſe developpe et paroit auſſi comme une fleche,

ſhaft of an arrow, ſee *f*, fig. 5. This is the immediate growth or production of the main ſtem already formed. The buds which adhere to the ſecond, third, and fourth row of the third ſet of roots likewiſe unfold and appear in regular ſucceſſion during each of the following weeks, if the rains continue. Still, however, theſe are only *ſecond productions*. The *third* are thoſe which come out of the ſecond in a month or five weeks after, *if the rains continue*, ſee *g*, fig. 5. But we muſt not forget to obſerve, with reſpect to the *ſecond productions*, that an exceeding favourable ſeaſon being neceſſary to their appearance, and this twelfth or fourteenth week (from the circumſtance of the plantation, being began in October or November) falling out in February or March, which is the uſual time of the greateſt droughts, they may be kept

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v. *f*, fig. 5. C'eſt une production propre de la ſouche déja formée: les boutons attachés au 2^e^, 3^e^, 4^e^, &c. plan de racines de ce 3^e^ ordre, ſe developpent auſſi et paroiſſent ſucceſſivement chacune des ſemaines ſuivantes *ſi la pluye continue*, mais ce ne ſont encore que des *ſecondes productions*. Les *troiſiemes productions* ſeront celles qui un mois ou cinq ſemaines après ſortiront de ces ſecondes, v. *g*, fig. 5. *ſi la pluye continue*. Mais ce qu'il ne faut pas oublier par rapport aux *ſecondes productions*, c'eſt qu'elles ont beſoin pour paroître d'une ſaiſon très avantageuſe, et que cette 12^e^ ou 14^e^ ſemaine dans laquelle elles doivent paroître ne tombant eu egard aux plantations de Novembre et Decembre, qu'en Fevrier où Mars, tems du plus grand ſec qui ſuſpend préſque entierement la vegetation dans la terre.

kept back, and only appear the May or June following, after the rainy ſeaſon has ſet in.

The fifth figure repreſents the bud (*b*) which adheres to the firſt row of the third ſet of roots, quite come out, and appearing above ground like the ſhaft of an arrow *f*. This is the ſecond production. The ſame figure likewiſe repreſents the bud (*c*) unfolded and appearing in *g*. This is the third production. It comes out of the firſt row of roots of the plant *f*, which muſt be conceived in a more advanced ſtate of vegetation than it is in the figure.

The time of, and requiſites neceſſary for, the ſucceſſive appearances of all theſe different vegetations, ſeem to me ſo many proofs of the fundamental part of my method, to wit, *the neceſſity of planting in the only ſeaſon fitted to accelerate and preſerve them.*

The

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decouverte, ces *ſecondes productions* retenües dans la terre ne paroîtront donc qu'en May ou Juin ſuivant, lorſque les pluyes ſeront decidées.

La figure 5 preſente le bouton (*b*) du premier plan du 3^e^ ordre de racines developpé et paroiſſant comme une pointe de fleche *f* hors de terre que nous ſuppoſons *a*. C'eſt la ſeconde production. La même figure preſente auſſi le bouton (*c*) developpé, et paroiſſant en *g*. C'eſt la 3^e^ production, elle ſort du premier plan de racines de la plante *f* qu'on doit ſuppoſer plus avancée qu'elle n'eſt repreſentée ici.

Le tems et les conditions du developpement des ces differentes productions me paroiſſent autant de preuves de la partie fondamentale de mon ſyſteme, ſçavoir, *la neceſſité de planter dans l'unique ſaiſon qui ſoit propre à hater et entretenir ces developpements.*

The nine months during which you may reaſonably look out for a continuation of rain are from the 15th of May to the 15th of February. The progreſſion of the rain keeps, as it were, pace with that of your canes when they are planted in May. Moderate in the beginning, more conſiderable afterwards, very copious when your canes are big enough to have nothing to fear from them, they leſſen gradually as the time of the crop draws near.

7thly, Whenever I ſtubbed up the ſtumps of a plant which had been ten months in the ground, I always found a portion of the woody part of the plant very ſound: the medullary part was rotten. This made me think that every *planted* cane continued, for the whole time of its duration, to derive its nouriſhment not only from the roots of the *ſecond* and *third orders* (which may be looked upon

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Les neuf mois où l'on peut raiſonnablement eſpérer une continuation de pluye, ſont depuis le 15 May juſqu'au 15 Fevrier. Les pluyes augmentent pour ainſi dire avec vos cannes plantées en May; foibles d'abord, plus conſiderables enſuite, et par averſes lorſque vos cannes ſont aſſez grandes pour ne les plus redouter, ceſſant enfin par degrés à meſure que le tems de la coupe approche.

7°, Lorſque j'ay deraciné des ſouches qui provenoient d'un plan mis en terre depuis dix mois, j'ay toujours trouvé très ſaine une portion de la partie ligneuſe du plan; la partie médullaire etoit pourrie; et j'ay cru que chaque canne *plantée* continuoit donc pendant qu'elle ſubſiſtoit à tirer ſa nourriture non ſeulement du *ſecond* et du *troiſieme* ordre de racines qu'on peut regarder comme ſes racines propres,

upon as its own roots) but likewiſe from thoſe of the *firſt*, that is, from thoſe which had ſprung about the joint to which the button which produced it was faſtened. Fig. 6. repreſents an entire cane (*a*) ſprung from the plant *b*, the woody part *c c*, was as ſound at the end of ten months as when it was put into the ground: the medullary part was entirely rotted as was likewiſe the herbaceous, *dddd*, which forms the top of the head. There is no doubt but the medullary part affords the bud the ſame nouriſhment as the lobes of almonds do to to the germ which they contain.

8thly, Whenever I ſtubbed up the ſtumps of a plant which had been thirteen months in the ground, and the canes of which had been cut a month or two before, I always found all the woody part of the plant which belonged to the ſtump rotten, the roots of the plant dried up,

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pres, mais encore du *premier* ordre de racines, c'eſt à dire de celles qui s'etoient formées à l'entour du nœud auquel etoit attaché le bouton qui l'a produite. La fig. 6. preſente une canne entiere *a* provenant du plan *b*, dont à dix mois la partie ligneuſe *cc* etoit auſſi ſaine que lors qu'on l'avoit miſe en terre, la partie medullaire etoit entierement pourrie, ainſi que la partie herbacée *dddd* qui compoſe la partie ſuperieure de la tête: la partie medullaire fournit ſans doute au bouton la nourriture que les lobes des amandes fourniſſent au germe qu'elles contiennent.

8°, Lorſque j'ay deraciné des ſouches qui provenoient d'un plan mis en terre depuis 13 mois, et dont on avoit coupé les cannes depuis un ou deux mois, j'ay toujours trouvé pourrie toute la partie ligneuſe du plan d'ou provenoit la ſouche, les

up, each of the young rattoons provided with its *ſecond* and *third* ſet of roots, and each of theſe *rattoons* faſtened in the ground to the joint of one of the canes which had been cut. This joint being ſtill green, I concluded, from the ſame kind of reaſoning, that theſe rattoons likewiſe drew their nouriſhment not only from their own proper roots, but likewiſe from thoſe of the joint they had come out of, juſt as the planted cane draws it both from its own roots, and from thoſe of the joint it comes from.

But as the plant has been always found rotten ſoon after the cane which it had produced had been cut, I began to ſuſpect, that canes of all ſorts, not only the *planted* but the *rattoons* likewiſe, never draw any nouriſhment but from

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les racines de ce plan deſſechées, chacun des nouveaux rejettons en poſſeſſion de ſon *ſecond* et *troiſieme* ordre de racines, et chacun de ces rejettons attaché dans la terre à un nœud d'une des cannes qu'on avoit coupées, ce nœud etant encore verd, j'ay conclu que les *rejettons* jouiſſoient auſſi non ſeulement de leurs propres racines, mais encore de celles du nœud dont ils etoient ſortis, comme la canne plantée jouit de ſes propres racines et de celles du nœud dont elle tire ſon origine.

Mais comme le plan, ſuivant ce qu'on vient de voir s'eſt toujours trouvé pourri, peu de tems après que la canne qu'il avoit produite a été coupée, j'ay ſoupçonné que toutes les cannes non ſeulement *plantées* mais même *rejettons* ne joüiſſoient jamais que des trois ordres de racines dont j'ay déja parlé: ce qui ſeroit

from the three ſets of roots of which I have ſpoken. This, if true, is a ſtrong argument againſt the pretended antiquity of the ſtools, which are ſaid to be ten, fifteen, and even twenty years old. It is alſo a very powerful argument in favour of my principle of the inutility of replanting when the ſtump is not raiſed above the ground, ſince grant my ſuſpicion to be grounded, and it follows, *that the oldeſt of the roots of a ſtool, the canes of which are cut every year, cannot be above two years old.*

9thly, In order then to clear up ſo important a point, I ordered a ſtool of canes, which had been planted about four years before, had been regularly cut every year, and for the laſt time about ſix months before, to be ſtubbed up. The rattoons were very fine; there were upwards of eighty of them, ſmall and great (hardly a ſixth part of this number uſually ſucceeds); the *group*, notwith-

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roit bien contraire à la vetuſté pretendue des ſouches qu'on dit être de 10, 15 et 20 ans: et ſeroit un grand argument en faveur de mon principe de l'inutilité de replanter, lorſque la ſouche n'eſt pas elevée au deſſus de la terre; puiſque dans le cas où mon ſoupçon ſeroit juſte, *la plus vieille de toutes les racines d'une ſouche dont on coupe les cannes tous les ans, n'auroit pas au dela de deux années.*

9°, Pour eclaircir un point auſſi important j'ay fait déraciner une ſouche de cannes plantées depuis quatre ans, coupée reguliérement chaque année, et pour la derniere fois depuis ſix mois: les rejettons etoient fort beaux, il y en avoit plus de 80 tant grands que petits (il en reuſſit rarement la ſixieme partie) la touffe-

notwithſtanding, was not above ten inches in diameter when it came out of the ground. In order not to injure it, I ordered ſix inches of the ſurrounding earth to be taken up with it. When I had had it waſhed and freed from all extraneous earth, I found no veſtiges of any wood but that of the canes which had been laſt cut: I then took each rattoon apart, and found it faſtened to a joint of theſe laſt canes, which joint was ſtill green. I examined the remains of each of theſe laſt canes, and found the kind of bulb, ſee *u*, fig. 1. by which they had been faſtened to ſome joints of canes of the preceding year, entirely *dried up*. I met with no appearance of this laſt cane, which conſequently had rotted, and in place of it there was nothing to be ſeen but dry roots; roots take a great while to rot.

Conclude

touffe n'avoit cependant pas au dela de dix pouces de diametre à ſa naiſſance hors de terre; pour ne la point endommager, j'ay fait cerner et enlever avec elle ſix pouces de terre à l'entour, et après l'avoir lavée et debarraſſée de toute ſa terre, je n'ay vu de trace d'aucun autre bois que de celuy des dernieres cannes coupées; j'ay detaché chaque rejetton ſéparément, et j'ay vu qu'il etoit attaché à un nœud de ces dernieres cannes, et que ce nœud etoit encore verd; j'ay ſuivi les reſtes de chacune de ces dernieres cannes, et j'ay vu entiérement *deſſeché* l'eſpece d'ombilic (v. *u*, fig 1.) par lequel ils etoient attachés aux nœuds de quelques cannes de l'année précédente; je n'ay vu aucune apparence de cette ancienne canne, qui par conſéquent etoit alors pourrie, on ne voyoit à ſa place que des racines deſſechées, les racines ſont aſſez long tems à pourrir.

Concluons

Conclude we then, as I ſuſpected, that the rattoons as well as the planted canes derive nouriſhment only from the three orders of roots of which I have ſpoke, with this difference only, that the firſt order of roots belonging to the *planted canes* comes from the plant which is put under ground; whereas that of the rattoons comes from the part of the cut canes which continue in the ground after they have been cut, and which rots the following year ſoon after the ſucceeding rattoons have appeared, ſo that the third order of roots of a cane which has been juſt cut becomes the firſt order of thoſe of the rattoon it is about to produce; which firſt order, as well as the ſecond, will rot the following year, when new rattoons ſhall have taken the place of thoſe which ſhall then be cut.

The

Concluons comme je l'avois ſoupçonné que les rejettons ne jouïſſent comme les cannes plantées que des trois ordres de racines dont j'ay parlé, avec cette difference que le premier ordre de racines des cannes plantées provient du plan mis en terre, et que celuy des rejettons provient de cette partie des cannes coupées qui reſte dans la terre, partie qui pourrira l'année ſuivante peu de tems après que les rejettons ſubſequens auront paru; de ſorte que le troiſieme ordre de racines d'une canne qu'on vient de couper, devient le premier ordre de racines du rejetton qu'elle va produire, lequel premier ordre pourrira l'année ſuivante ainſi que le ſecond, aprés que de nouveaux rejettons auront ſuivi la coupe qu'on aura faite des precedents.

The idea, therefore, of having ſtumps of fifteen or twenty years ſtanding muſt be given up, ſince it is proved, that the oldeſt are thoſe of the laſt year; but this idea being given up, it can never be aſſigned as a reaſon for re-planting ſo often. It will be ſufficient, therefore, to replant annually a ſixth of one's land: to ſpeak more correctly, it will always be uſeleſs to replant, *as long as the ſtump is not raiſed above ground, and the ground about it is raiſed as it ought to be by annual and well-directed labours.* I ſay, the ground about muſt be annually raiſed, for otherwiſe it is impoſſible but that the feet of the negroes and cattle muſt harden and render it impervious to the roots which are to make their way every year.

It likewiſe appears of what conſequence it is to cut the canes in the ground in order to prevent the ſtump from

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Il faut donc renoncer à la pretention d'avoir des ſouches de 15 et 20 ans, les plus vieilles ſont de l'année precedente, et cette prétendue vetuſté des ſouches etant demontrée fauſſe, elle ne doit plus être une raiſon pour replanter auſſi ſouvent; il ſuffira donc de replanter annuellement la ſixieme partie de ſa terre, ou pour mieux dire, il ſera toujours inutile de replanter *lorſque la ſouche ne ſera pas elevée au deſſus de la terre et que par des labours annuels et bien faits on aura ſoin de ſoulever celle qui l'environne,* que les pieds des negres et des beſtiaux quand on la travaille ne manqueroient pas d'affaiſſer à la longue, et de rendre preſque impénétrable aux nouvelles racines qui doivent ſe former chaque année.

On voit auſſi combien il eſt eſſentiel de couper les cannes dans la terre, afin

from riſing above ground, conſequently how bad the method of moulding up the ſtumps, thought by ſo many people to be ſo good, really is; ſince it raiſes the ſtump every year, and ought, therefore, only to be adopted with reſpect to thoſe pieces one propoſes to replant the next year: then, indeed, it is excellent, but in that ſingle caſe only.

Hence likewiſe it appears, why rattoons are forwarder at the twelvemonth's end than planted canes are at the end of thirteen. The firſt order of the roots of the rattoons is in its full vigour at the inſtant of the crop; that of the planted canes is ſeldom ſo at a month's end.

We likewiſe ſee why the rattoons of canes that have been cut at the end of ten, eleven, or twelve months, are always finer than thoſe of canes which have ſtood fifteen or ſixteen months; the woody part of the latter being much

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d'empêcher la ſouche de s'élever; et combien eſt mauvaiſe la maxime de rechauſſer, que tant de gens trouvent ſi bonne, qui eleve cependant tous les ans la ſouche, et qu'on ne devroit ſuivre que par rapport aux piéces qu'on ſe propoſe de replanter l'année ſuivante, elle eſt excellente alors, mais dans ce cas ſeulement.

On voit auſſi pourquoy les rejettons a douze mois ſont plus avancés que les cannes plantées ne le ſont à treize: le premier ordre de racines des rejettons eſt dans toute ſa force au moment de la coupe, celuy de cannes plantées l'eſt rarement au bout d'un mois.

On voit auſſi pourquoy les rejettons des cannes coupées a dix, onze, et douze mois ſont toujours plus beaux que ceux des cannes coupées a 15 et 16 mois, le

much harder is exactly in the ſame caſe as the plant, the firſt joints of which (as I obſerved before) produced no ſhoots, except the ſeaſon happened to be remarkably favourable.

Hence likewiſe we ſee, why one never gets any fine rattoons from the grounds which are called exhauſted: the planted canes in them are cut at the end of the fifteenth or ſixteenth month; at which time the part of their wood which remains under ground, and out of which the rattoons come, is extremely hard. Beſides, this is done in February, March, or April, months, as may be ſeen from the account I have given before of the ſeaſons, not likely to produce miracles of vegetation.

Hence, finally, we ſee, why it is not at all to be wondered at, that the rattoons which I cut before the time ſhould

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bois de celles cy beaucoup plus dur eſt dans le cas du plan dont j'ay remarqué cy deſſus que les premiers nœuds qui etoient les plus durs, ne donnoient point de jets à moins d'une ſaiſon ſingulierement favorable.

On voit auſſi pourquoy l'on n'a jamais de beaux rejettons dans ces terres qu'on appelle epuiſées; on y coupe les cannes plantées a 15 et 16 mois, la partie de leur bois qui reſte en terre, et d'ou proviennent les rejettons eſt alors très dure, et d'ailleurs cette coupe ſe fait en Fevrier, Mars, et Avril, et l'on peut voir par le tableau de la pluye, p. 217. que ces trois mois ne ſont pas propres à produire des miracles de végétation.

On peut conclure auſſi qu'il n'eſt pas etonnant que les rejettons dont j'ay toujours anticipé la coupe aient été (comme je l'ay éprouvé) ceux qui ſe ſoient le

ſhould have been what I ever found them to be, thoſe which ſtood the beſt; that nothing, on the contrary, ſo much improves a piece of rattoons as cutting them before the time; and that, therefore, this operation (a neceſſary one when my method is followed) is not without its attendant advantage, which perhaps much leſſens the loſs which I have ſuppoſed to ariſe from it in page 213.

10thly, Having waſhed and gently ſcraped as far up as their beginning (that is, to the kind of bulb I deſcribed) two rattoon canes, ſix months old; the one taken out of a damp, the other out of a very dry ſoil; the one with fourteen joints above ground, on a length of twenty-eight inches; the other with only two joints, on a length of two inches; I found that the cane which had fourteen joints above ground, had only five rows of roots of the *third order* on a length of an inch and a half under ground;

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mieux ſoutenus; que rien n'eſt plus capable de reparer une piece de rejettons que d'en anticiper la coupe; et qu'ainſi l'anticipation des coupes neceſſaire dans mon ſyſteme, n'eſt point ſans un avantage qui diminue peut-être bien conſidérablement la perte que j'ay ſuppoſée, p. 213.

10°, Après avoir lavé et grattê legerement juſqu'à leur origine (juſqu'à cet eſpéce d'ombilic dont j'ay parlé) deux cannes rejettons de ſix mois, l'une priſe dans un terrein humide, l'autre dans un terrein très ſec, l'une ayant hors de terre quatorze nœuds dans une longueur de 28 pouces, l'autre ayant ſeulement cinq nœuds dans une longueur de deux pouces, j'ay vu que la canne qui avoit quatorze nœuds hors de terre, n'avoit en terre que cinq plans de racine du *troiſieme ordre* dans une longueur

ground; whereas the cane which had only five joints out of the ground had fourteen rows of roots, likewiſe of the *third order*, on the ſame length as the others.

This obſervation proves, that to the time of the canes jointing, that is, to the appearance of one of its joints above ground, the drying of each external leaf is conſtantly followed by a row of roots of the third order. One ſees too, why canes that are in moiſt ſoils joint out of the ground at the three months end; but have then only four or five rows of roots of the third order, diſtant from each other three, four, and five lines under ground; whereas, on the contrary, in dry lands, or, what comes to the ſame, in a ſoil remarkably impoveriſhed for want of cultivation, the canes do not joint above ground before the five months; but then they have under ground

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longueur d'un pouce et demi, et que la canne qui n'avoit que cinq nœuds hors de terre, avoit en terre quatorze plans de racines auſſi du *troiſieme ordre* et dans la même longueur.

Cette obſervation prouve que juſqu'au tems où la canne noue c'eſt à dire laiſſe voir un nœud hors de terre, le deſſechement de chaque feuille exterieure, eſt toujours ſuivi d'un plan de racines du troiſieme ordre; et l'on voit pourquoy dans les terreins humides, les cannes nouent hors de terre a 3 mois, mais alors elles n'ont en terre que 4 à 5 plans de racines du troiſieme ordre diſtants les uns des autres de 3, 4, et 5 lignes; dans les terres ſeches au contraire, ou (ce qui eſt egal) ſinguliérement affaiſſées par le defaut de culture, les cannes ne ſont noüées hors de terre qu'à cinq mois, mais alors elles ont en terre quatorze à quinze

ground fourteen or fifteen joints or rows of roots, diſtant from each other only a line, and ſometimes leſs; the joints, therefore, of the canes, of a dry or ill cultivated ſoil, are as many in number as thoſe of a moiſt and fruitful ſoil; but the two differ in this: the greater part of the former are ſhort, ſmall, and in the ground; whereas the greater part of the laſt are thick, long, and above ground.

It is therefore the excellence or the defect either of the ſeaſon, or the cultivation, which keeps back and baſtardizes the one ſort, which haſtens and makes the other thicker: chuſe, therefore, that ſeaſon for planting in which the greateſt poſſible ſucceſſion of rain ſhall force *out* of the grounds thoſe joints which, in a drier ſeaſon, would only have formed a quantity of roots within it, the

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quinze nœuds ou plans de racines diſtants les uns des autres d'une ligne ſeulement et quelque fois moîns: les nœuds des cannes d'un terrein ſec ou mal cultivé ſont donc égaùx en quantité à ceux des cannes d'un terrein humide ou fertile, mais ils différent en ce que la plus grande partie des uns ſont courts, petits, et dans la terre; le plus grand nombre des autres ſont gros longs et hors de terre.

C'eſt donc la bonté ou le defaut de la culture ou de la ſaiſon qui retient et abâtardit les uns, precipite et groſſit les autres; choiſiſſez donc pour planter la ſaiſon ou la plus grande continuité de pluye poſſible, forcera le plutôt à ſe developper hors de terre, les nœuds qui dans une ſaiſon plus ſeche formeroient uniquement en terre, une quantité conſidérable de racines dont l'inutilité eſt viſible

the uſeleſſneſs of which appears from the ſtate of the lands which are burthened with them.

The firſt reaſon for planting in May or June (ſee obſervation the ſixth, p. 227.) reſpected the coming up of the different productions, firſt, ſecond, &c. in general; that I have juſt given regards the firſt appearances of each particular production. We will next conſider if my method is equally favourable to thoſe which are to give the plant all the degree of perfection it is capable of acquiring.

Hiſtory

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viſible par l'etat des plantes qui en ſont affligées.

La premiere raiſon de planter en May et Juin, dont j'ay parle obſ. 6. p. 227. intereſſoit la ſortie des différentes productions en général premiere, ſeconde, &c. la ſeconde raiſon dont je viens de parler intereſſe les premiers developpemens de chaque production en particulier, voyons ſi mon ſyſteme favoriſe egalement ceux qui doivent donner à la plante toute la perfection dont elle eſt ſuſceptible.

Hiſtoire

History of the joints of the cane above ground.

With reſpect to the joints of the cane, there are two points of equal conſequence to be conſidered, the number and the quality.

With regard to number, the calculation of it, in both the methods, depends upon ſo many conſiderations, that I think myſelf obliged to reduce the queſtion to its moſt ſimple ſtate, by barely declaring, that I now cut all my canes every year, inſtead of only cutting three-fourths of them, as I uſed to do when I purſued the other method. Thoſe, however, who wiſh to know more of it, may ſee in another part of this paper the only calculation the matter is capable of.

As

Hiſtoire des nœuds de la canne hors de terre.

Il ſe preſente à l'égard des nœuds de la canne deux points à conſiderer auſſi eſſentiels l'un que l'autre, le nombre et la qualité.

Quant au nombre le calcul qu'on peut en faire dans les deux ſyſtemes, eſt ſubordonné à tant de ſuppoſitions que je crois devoir ſimplifier la queſtion, en expoſant que je coupe maintenant toutes mes cannes chaque année au lieu den couper ſeulement les trois quarts comme je faiſois, lorſque je ſuivois l'autre ſyſteme. Voyez cependant dans ſon lieu, l'unique eſpece de calcul dont je croye la matiere ſuſceptible.

As to the quality of the joints; this, I think, is to be decided by the hiſtory of them, to which hitherto ſcarce any attention has been paid, though the ſlighteſt would have been ſufficient to have ſhewn the abſurdity of the received ideas concerning the age which muſt be allowed the canes, in order for them to arrive at perfect maturity.

At the end of the third month, ſometimes later, as this depends upon the ſoil and ſeaſon, nothing yet appears but a head covered with leaves: ſee figure 8. from *a* to *z*, which is a ſmall ſheath containing a number of white leaves which come out by degrees. The leaf *b*, ſee fig. 9. falls at length, and diſcovers a firſt joint above ground. From that inſtant to the time of its full growth it acquires four or five joints every month: theſe alſo de-

pend

Quant à la qualité des nœuds il me ſemble qu'elle peut être decidée par leur hiſtoire à laquelle on n'a fait juſqu'à preſent qu'une mediocre attention, quoique la plus legere eut ſuffi pour demontrer la fauſſete des idées qu'on a eu juſqu'à preſent ſur l'age qu'il faut donner aux cannes, pour leur procurer une maturité parfaite.

A trois mois quelquefois plus tard (ſuivant la terre et le tems qu'il a fait) la canne ne preſente encore qu'une tige herbacée garnie de feuilles, v. fig. 8. depuis *a* juſqu'à *z* qui eſt un petit rouleau qui renferme une quantité de feuilles blanches qui doivent ſe developper par degrés; la feuille *b*, v. fig. 9. tombe enfin et laiſſe voir un premier nœud hors de terre. De ce moment juſqu'à celuy où elle a pris tout ſon accroiſſement elle augmente chaque mois de quatre à cinq

nœuds

pend upon the weather and ſoil, and are longer or ſhorter, thicker or thinner, according as thoſe are more or leſs favourable. One may ſee (figure 1, from *i* to *k*) the effects of a continued drought when it ſets in in February, and laſts till May or longer. The joints are ſtunted, and inſtead of being an inch and a half or two inches long, like thoſe from *h* to *i*, which come out in a favourable ſeaſon, ſome of them are hardly two lines, and ſome even leſs. When the rains begin again and continue, the joints are longer again (ſee *k* to *l*) and like the firſt from *h* to *i*. The effects of the dryneſs of the ſoil are exactly ſimilar to thoſe of the weather. I once had a cane in my plantation worthy a place in a collection of natural hiſtory: it was of the thickneſs of a pen, and only three inches long; notwithſtanding

which

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nœuds plus ou moins gros, plus ou moins longs, c'eſt auſſi ſuivant le tems et le ſol, ou peut voir fig. 1. depuis *i* juſques à *k* l'effet d'un ſec tres vif lorſqu'il commence avec le mois de Fevrier et qu'il continue juſqu'en May ou plus long tems; les nœuds ſont etranglés et au lieu d'avoir un pouce et demi ou deux pouces de long, comme les nœuds depuis *h* juſqu'a *i* produits dans une bonne ſaiſon, quelques uns de ceux produits dans le tems du ſec, ont a peine deux lignes, d'autres en ont moins: lorſque les pluyes recommencent et continuent, les nœuds qui ſortent ſont plus longs, comme on les voit depuis *k* juſqu'a *l*, et tels que les premiers depuis *h* juſqu'a *i*. L'effet de l'avidité du ſol eſt exactement le même que celuy de la vivacité et de la longueur du ſec: j'ay vu chez moy dans une eſpéce de tuff près d'une carriére, une canne digne d'être miſe dans le cabinet d'un curieux, elle etoit groſſe comme une plume à ecrire, longue de trois pouces, et n'en

which it had its two and twenty joints very diſtinct. I ſent it in a letter to a perſon who might have ſent me in exchange ſome ſix feet long; which, however, would not have had above twenty-two joints any more than mine, if the firſt joints of both had come out of the earth at the ſame time.

The firſt joint, which comes out either at the third, fourth, or fifth month, always keeps in its firſt place near the earth, ſee *a*, fig. 9.; out of this firſt comes the ſecond, ſee *b*, fig. 10.; and out of the ſecond a third, after the leaf *cc* has dried, and is fallen off like the leaves *aa*, *bb*. Each week produces its joint, or very nearly: a leaf likewiſe dries and falls off pretty nearly every week. A cane of thirty-two joints, ſuppoſe fit to cut, has from five to eight and twenty of them which have loſt their leaves; the next five or ſix ſtill have them, but withered

and

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n'en avoit pas moins ſes 22 nœuds tres diſtincts; je l'envoiai dans une lettre à une perſonne qui auroit pu m'en envoyer en echange quelques autres de 6 pieds de long et qui dans cette longueur n'auroient eu que 22 nœuds comme la mien ne ſi elles avoient noué hors de terre en même tems.

Le premier nœud qui paroit ſoit à 3 ſoit à quatre ou à cinq mois, reſte toujours à ſa même place près de la terre, v. *a*, fig. 9. de celuy la ſort le ſecond, v. *b*, fig. 10. du ſecond le troiſieme, apres que la feuille *c*, *c*, ſera deſſechée et tombée comme les feuilles *aa*, *bb*, même figure, chaque ſemaine fournit environ ſon nœud; il ſe deſſéche et tombe donc à peu près une feuille par ſemaine; dans une canne de 32 nœuds, je ſuppoſe, bons à couper, on en voit 25 à 28 depouillés naturellement de leurs feuilles, et cinq à ſix autres garnis encore de

and ready to fall off: the remaining joints, ſurrounded with green leaves, form the head, which one takes care to cut off after the laſt leaf has withered. The bare recital of facts ſeems fully to prove that the withering and fall of a leaf is the only criterion (and a ſufficient one it is) of the maturity of the joint to which it adhered; and that the eight laſt joints of two canes, which are cut the ſame day, have exactly the ſame age and the ſame degree of ripeneſs, notwithſtanding one of the canes themſelves may be fifteen, and the other only ten months old. My reaſoning is further confirmed by the experiment repeatedly made on two equal quantities of canes, cut the ſame day in pieces of a different age; it was evident, that each joint of the cane, of a ſuppoſed growth of ten months, contained the ſame quantity of ſugar as that of a cane of the ſuppoſed growth of fifteen.

The

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leurs feuilles deſſechées prêtes a tomber; les nœuds ſuivant, garnis de feuilles vertes, forment la tête qu'on a ſoin de couper après la derniere feuille deſſechée: la ſeule expoſition des faits me ſemble demontrer que le deſſechement et la chute d'une feuille, eſt l'unique preuve (et preuve ſuffiſante) de la maturité du nœud au quel elle etoit attachée: et que les huits derniers nœuds de deux cannes coupées le même jour ſont exactement du même age et de la même maturité, quand bien même l'une de ces cannes auroit quinze mois et que l'autre n'en auroit que dix; preuve de raiſonnement confirmée par des expériences réiterées de deux quantités egales de cannes, priſes dans des piéces d'un age different, et coupées le même jour, par leſquelles il conſte que chaque nœud de cannes pretendües de dix mois contient autant de ſucre que chaque nœud de cannes pretendües de quinze.

La

The neceſſity, therefore, which my method lays me under of cutting my planted canes at the twelvemonth's end, and my rattoons at eleven, has nothing in it which contradicts the proper idea which ought to be entertained of the abſolute maturity of this plant. This is decided by experience, and the hiſtory of its joints.

With regard to its relative maturity, that which is of conſequence to the ſugar, this does not depend on the age, but on the ſeaſon. In February, March, and April, all the canes, whatever be their age, are as ripe as the nature of the ſoil ever allows them to be, and accordingly I never fail to make the greateſt part of my ſugar at this ſeaſon.

It will be of uſe to obſerve, that in the ſame week in February I cut in a piece which had been planted ten months a quantity of canes ſufficient to yield me four thouſand

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La neceſſité ou je ſuis dans mon ſyſteme de couper mes cannes plantées à douz. mois et mes réjettons a onze, n'a donc rien de contraire à l'idée qu'on doit ſe faire de la maturité abſolüe de cette plante, maturité decidée par l'expérience auſſi bien que par l'hiſtoire de ſes nœuds; quant à ſa maturité relative, celle qui intereſſe le ſucre elle n'eſt pas l'effet de l'age mais de la ſaiſon, en Fevrier, Mars, et Avril, toutes les cannes de quelque age quelles puiſſent être, ont la perfection de maturité dont la qualité du terrein les rend ſuſceptibles, et je ne manque pas de faire la plus grande partie de mon ſucre dans cette ſaiſon.

Il ne ſera pas inutile d'obſerver que j'ay coupé dans la même ſemaine de Fevrier, dans une piece agée de 10 mois, la quantité de cannes neceſſaires pour me

thouſand gallons of juice, and exactly the ſame quantity in a piece which had been planted fifteen months. If any body ſhould chance to repeat this experiment, they ſhould be careful to chuſe the ſame kind of ſoil, the ſame expoſition, and the ſame time of year. The juice of the piece which was ten months old was a little greeniſh, that of the piece of fifteen months was exceeding brown. The four thouſand gallons of juice from each of the pieces gave me the ſame quantity of very fine ſugar, which proves that each joint of canes ſuppoſed to be nine months old contains juſt as much ſugar as each joint of a cane to be fifteen months old.

It likewiſe proves, that all the joints, whoſe leaves have dried and are fallen off, have acquired that maturity which I call abſolute.

It

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me donner quatre mille galons de *veſou* ou jus, et la même quantité dans une piece agée de 15 mois (ſi l'on vouloit reiterer la même experience il faudroit choiſir dans les deux piéces a peu près la même qualité des terre et la même expoſition comme on choiſirait le même tems) le veſou de la piece agée des 10 mois etoit un peu verdâtre, celuy de la piéce de 15 mois etoit fort brun; les 4000 galons de veſou de l'une et de l'autre piéce m'ont donné la même quantité de très beau ſucre. Ce qui prouve que chaque nœud de cannes pretendües de dix mois contient autant de ſucre que chaque nœud des cannes prétendües de quinze.

Cela prouve auſſi que tous les nœuds dont les feuilles ſont deſſechées et tombées ont acquis cette maturité que j'appelle abſolüe.

Cela

It likewiſe proves, that it is not the age of the canes but the dryneſs of the weather, which increaſes from January to April, which is the cauſe that in January four hundred gallons of juice commonly yield forty-eight gallons of ſugar and molaſſes, one with another; in February from fifty-ſix to ſixty-four; in March from ſixty-four to ſeventy-two; in April ſometimes eighty; after which period the ſugar ferments, and even burns when the refiner happens not to be very expert at his buſineſs. Hence I concluded, that the greateſt perfection of relative maturity to which my canes could arrive was when the juice of them was made up of four parts water, and one part conſiſting of part ſugar and part molaſſes.

This laſt obſervation likewiſe proves, that the colour of the juice has nothing to do with either the quantity

or

Cela prouve auſſi que ce n'eſt pas l'age des cannes mais le ſec qui augmente de Janvier en Avril qui fait qu'en Janvier 400 gallons de veſou rendent ordinairement 48 gallons tant ſucre que melaſſe, en Fevrier de 56 a 64; en Mars de 64 a 72; en Avril quelquefois 80. Après quoy le ſucre mouſſe, brule même, ſi le raffineur n'a qu'une routine; de ce dernier point j'ay conclu que la plus grande perfection de maturité *relative* ou mes cannes puſſent atteindre, etoit lorſque leur jus contenoit quatre parties d'eau et une tant de ſucre que de melaſſe.

Cela prouve auſſi que la couleur du veſou n'intereſſe ni la quantité ni la qua-

lité

or the quality of the ſugar, but may ſerve as an indication of the manner in which it is to be made. I ſhould think an object of this importance would not be unworthy the attention of ſome very great chemiſt.

Other obſervations have ſhewn me that in canes which are perfectly ripe, the quantity of the ſugar is equal to that of the molaſſes.

Let us now examine ſome other circumſtances relative to the growth of the cane, and particularly that of its duration.

The

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lité du ſucre, mais elle ſert d'indice pour la traiter d'une façon ou d'un autre, et il me ſemble qu'un objet de cette importance ne ſeroit pas indigne de l'etude d'un très grand chimiſte.

D'autres obſervations m'ont prouvé que dans les cannes parfaitement mures la quantité du ſucre eſt egale à celle de la melaſſe.

Examinons quelqus autres circonſtances de l'accroiſſement de la canne et ſur tout celle de ſa durée.

The account of the cane in different ſoils, together with that of the different degrees and different kinds of growth they give to it.

We are not to form our ideas of what a plant is in a country favourable to it, from what we ſee it in one where all it reaps from the care beſtowed on it, is the lengthening out of a wretched exiſtence uſeleſs to every purpoſe but curioſity. The cane to be met with in the hot-houſes of Europe is the bare ſhadow of the American cane. Let us not then eſtimate the natural duration of the one by the artificial, and in ſome meaſure only apparent duration of the other: I with juſtice call it an apparent duration; for, if the European cane acquires only

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Hiſtoire de la canne dans les differentes eſpéce de terre, des divers degrés, et de l'eſpéce d'accroiſſement qu'elle y acquier.

Il ne faut pas juger de l'hiſtoire d'une plante dans un climat qui favoriſe tous ſes developpemens, par ſon état dans un pais ou elle ne retire d'autre fruit des ſoins qu'on luy donne, que la prolongation d'une triſte exiſtence, inutile à tous autres egards que celuy de la curioſité. La canne qu'on voit dans les ſerres en Europe n'eſt que l'ombre de celle d'Amerique; ne jugeons donc point de la durée naturelle de l'une, par la durée artificielle et en quelque façon apparente de l'autre; je dis, apparente, car ſi la canne d'Europe ne croît, je ſuppoſe que d'un

only a ſingle joint each year, fifty-two years of exiſtence in Europe are not more than fifty-two weeks exiſtence in America[a].

RECITAL OF FACTS.

1ſt, I have never ſeen, either in my own plantation or that of any other perſon, a cane which had more than

(a) When I firſt made this ſuppoſition, I rather conjectured what muſt be than what I knew exactly; but ſince that time, Mr. THOUIN, of the King's Botanic Garden at Paris, has rid me of the little anxiety I could not help feeling at having ventured an aſſertion of the truth of which I was not accurately ſure. He ſhewed me a cane which, *he believes*, to have been planted twelve years ago: it has thirty joints. In order to be convinced that it has grown at the rate of two joints and a half a year, one ſhould be fully ſatisfied that it was planted twelve years ago; but he likewiſe ſhewed me one which *he knows* was brought from America in a pot ten years ago, and this has only two joints out of the ground. Conclude we then, as I ſaid before, that, in order to know a plant thoroughly, we ſhould ſtudy it in the climate to which it belongs.

forty

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d'un nœud par chaque année, 52 années d'exiſtence en Europe n'équivaudroient qu'à 52 ſemaines d'exiſtence en Amerique *.

EXPOSONS DES FAITS.

1^e obſervation, Je n'ay jamais vu chez moi ni ailleurs de cannes qui euſſent

* Lorſque je hazardois cette ſuppoſition je ne faiſois qu'imaginer ce qui pouvoit être; Mr. THOUIN, du Jardin du Roy à Paris, m'a mis en etat d'apprecier une idée ſur laquelle je n'etois pas ſans une eſpéce d'inquietude; il m'a fait voir une canne *qu'il croit* plantée il y a douze ans: elle a trente nœuds; pour etablir qu'elle a cru a raiſon de deux nœuds et demi par an il faudroit être certain qu'elle a été plantée il y a douze ans; mais il m'a fait voir auſſi une canne *qu'il ſcait* avoir été apportée d'Amerique dans un pot il y a deux ans, elle n'a que deux nœuds hors de terre: concluons du moins comme je l'avoir dit que pour connoitre une plante, il faut la ſuivre dans le climat qui lui eſt propre.

forty or fifty uſeful joints (I do not ſpeak of thoſe of the head); nor have I often met with this number upon any, the length of which fell ſhort of ſeven, or exceeded nine feet.

2dly, I have never ſeen this length but either in a new ſoil, or in a ſoil the moiſteſt that could be without being quite drowned; that is to ſay, in a ſoil the moſt favourable to the quickeſt and greateſt vegetation of the cane.

3dly, In a ſoil of this kind I have always ſeen the firſt joint out of the ground at the three months end; if there had been frequent ſhowers and no hard rains, at the end of two and a half.

4thly, Even in ſuch a ſoil I have never cut canes at the thirteen months end without finding many of them rotten

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au dela de 46 nœuds utiles (je ne parle pas de ceux de la tête) et j'ay rarement vu cette quantité de nœuds dans une longueur moindre de ſept pieds, et plus conſiderable que neuf.

2^e^ obſervation, Je n'ay jamais vu cette longueur ailleurs que dans un terrein neuf, ou le plus humide ſans être noyé, c'eſt à dire dans le terrein le plus favorable au plus prompt et au plus grand accroiſſement de la canne.

3^e^ obſervation, Dans un terrein de cette eſpéce, j'ay toujours vu la canne nouée hors de terre, à trois mois, quelquefois à deux et demi, s'il y avoit eu des ondées frequentes ſans averſes.

4^e^ obſervation, Je n'y ay jamais coupé les cannes à 13 mois ſans en trouver beaucoup de pourries ou preſque deſſechées, pourries et couchées, ſi l'année avoit été

rotten or almoſt dried up; rotten and laid if there had been much rain in the year; almoſt withered, though ſtill ſtanding, if there had been very little.

Is it not ſafe to infer from theſe four obſervations, that forty-ſix joints are almoſt the *ne plus ultra* of the growth of the cane in moiſt ſoils? and again, that they are the produce of pretty nearly an equal number of weeks which elapſe after the appearance of the firſt joint? for it is impoſſible to think, that the canes continue growing till the time in which one finds many of them rotten; but if I am in the right it follows, that canes of the firſt production, which live after the twelfth or at moſt after the thirteenth month, live as men do who have paſt the age of forty, every day takes ſomething from their vigour. The ſecond productions are the

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été pluvieuſe, preſque deſſechées quoique ſur pied, s'il y avoit eu très peu de pluye.

Ne peut on pas conclure de ces quatre obſervations que ces 46 nœuds ſont à peu près le *non plus ultra* de l'accroiſſement des cannes dans les terrein humides, que ces 46 nœuds etoient le travail d'á peu près autant de ſemaines qui s'etoient ecoulées depuis la ſortie du premier nœud, puis qu'on ne peut ſuppoſer que les cannes croiſſent juſqu'au moment ou l'on en trouve beaucoup de pourries, et qu'ainſi l'on peut croire que les cannes de *la premiére production* qui ſubſiſtent encore après douze ou tout au plus 13 mois ſe ſoutiennent comme font les hommes après avoir atteint l'age de 40 ans, tous les jours avec quelque diminution

the only ones which are then in their perfection; the third and fourth have not yet reached it.

5thly, In a good ſoil, having a favourable expoſition, well drained and worked for a number of years, I have never ſeen any canes with above thirty-eight or forty joints; nor have I ever ſeen any that had ſo many when the length of the cane has fallen ſhort of four feet and a half, nor this except in pretty good years.

6thly, I have never ſeen the firſt joint of the cane appear in ſuch a ſoil ſooner than the fourth or the middle of the third month.

Nor, ſeventhly, have I ever cut ſuch canes at the fourteen or fifteen months end, but I found ſome that were either rotted or dried, according to the ſeaſon; rotted

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tion de leur valeur réelle: les *ſecondes productions* ſont les ſeules qui ſubſiſtent alors dans leur perfection; les *troiſiemes et quatriemes* ne l'ont pas encore atteinte.

5^e^ obſervation, Dans un bon terrein bien expoſé, egouté, et travaillant depuis pluſieurs années, je n'ay jamais vu de cannes qui euſſent au dela de 38 à 40 nœuds, ny cette quantité de nœuds dans une longueur moindre de 4 pieds et demi, et ces 4 pieds et demi avec leur 40 nœuds ſeulement dans des années aſſez favorables.

6^e^ obſervation, Je n'ay jamais vu la canne nouée dans cette eſpece de terrein avant le 4^e^ mois ou le milieu du 3^e^.

7^e^ obſervation, Je n'y ay jamais vu couper les cannes à 14 ou 15 mois ſans en trouver de pourries ou deſſechées ſuivant la ſaiſon, pourries ſi je les coupois après les

ted when I cut them after the January rains; dried when I let them ſtand the three dry months, and cut them in April.

May one not infer from the fifth and ſixth obſervations, that from thirty-eight to forty joints are nearly the *ne plus ultra* of the growth of the cane in a good ſoil, with a favourable expoſition? and that theſe thirty-eight to forty joints are the produce of a like number of weeks which elapſe from the appearance of the firſt joint, at the fourth month's end, or a fortnight ſooner? conſequently, that the only difference between the growth of the canes in ſuch a ſoil, and what it would have been in a moiſt one, is only that the former joint four or five weeks later than the laſt would have done.

And

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les pluyes en Janvier, deſſechées ſi je les coupois après trois mois de ſec en Avril.

Ne peut-on pas conclure de la 5e et 6e obſervation que les 38 à 40 nœuds ſont à peu près le non plus ultra de l'accroiſſement des cannes dans un bon terrein bien expoſé et egouté, que ces 38 à 40 nœuds ſont le travail de 38 à 40 ſemaines ecoulées depuis la ſortie du premier nœud à 4 mois, ou 3 mois et demi; et qu'ainſi, l'accroiſſement des cannes dans un pareil terrein ne differe de celuy qu'elles prendroient dans un terrein humide que des quatre ou cinq ſemaines qu'elles nouent plutôt dans celuy ci que dans l'autre.

And may not one infer from the fifth, ſixth, and ſeventh obſervations put together, that in a good ſoil, with a favourable expoſition, and well drained, the canes of the firſt production may live the fourteenth month through, perhaps, if they have got to the fourteenth in February, and the dry weather be not exceſſive, reach to the ſixteenth? but that their exiſtence from thirteen months to fifteen will be like that of a ſtout hale man between forty and five and forty, who is much miſtaken if he thinks himſelf as ſtrong then as he was at thirty-five? Whence it follows, that it muſt be advantageous to cut the canes, which grow in ſuch a ſoil, at the twelve month's end, if the other circumſtances of the method which has been adopted allow it; or even at the end of eleven months, if theſe circumſtances require it: becauſe in this laſt caſe one may expect a compenſa-

tion

Et ne peut on pas conclure de la 5^e^, 6^e^, et 7^e^ obſervations réunies que dans un bon terrein bien expoſé et egouté, les cannes de la premiere production peuvent bien ſe ſoutenir juſqu'à 14 mois peutêtre même juſqu'à 15, ſi elles ont atteint le quatorzieme en Fevrier et que le ſec ne ſoit pas bien vif, mais qu'il en ſera de leur etat depuis 13 mois juſqu'à quinze, comme de celuy d'un homme ſec et nerveux depuis 40 juſqu'à 45 ans, qui s'abuſe s'il croit être auſſi vigoureux qu'à 35. D'où il ſuit qu'il ne peut être qu'avantageux de couper dans un pareil terrein les cannes à douze mois ſi les autres circonſtances du ſyſteme qu'on aura adopté peuvent le permettre; et même à 11 mois, ſi les autres circonſtances l'exigent; parce que dans ce cas on peut eſpérer un dedommagement dans la

vigueur

tion in the additional vigour given to the ſtump, for the diſadvantages mentioned above by thus cutting before the uſual time.

Obſervation the 8th, In a dry but good ſoil, not manured, but well worked and ſeconded by the ſeaſon, I have never ſeen canes with more than from thirty to thirty-four joints, and theſe on a length of from three to four feet.

Obſervation the 9th, I have never ſeen the firſt joint of theſe come out ſooner than at the end of four months or four months and a half.

Obſervation the 10th, When I have cut theſe at the fifteen months end, I have always found them ſtanding, but very dry, and ſometimes a little changed.

May we not infer from the eighth and ninth obſervations, that from thirty to thirty-four joints is nearly the

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vigueur qu'on procure à la ſouche par une coupe anticipée.

8e obſervation, Dans un terrein ſec, quoique bon, point fumé, mais bien travaillé et tres aidé de la ſaiſon, je n'ai jamais vu de cannes qui euſſent au dela de 30 à 34 nœuds, et cette quantité dans une longueur de trois à quatre pieds.

9e obſervation, Je n'y ai jamais vu le premier nœud ſortir avant quatre mois, 4 mois et demi.

10e obſervation, Lorſque j'ai coupé ces cannes à 15 mois, je les ai toujours trouvées ſur pied, mais tres ſeches et quelquefois un peu altérées.

Ne peut-on pas conclure de la 8e et 9e obſervation que ces 30 à 34 nœuds ſont

ne plus ultra of the growth of the canes in a ſoil which is good, though dry; not manured, but pretty well worked, and ſeconded by the ſeaſon?

And may we not infer from the three obſervations put together, that theſe thirty to thirty-four joints are the work of as many weeks which have elapſed ſince the appearance of the firſt joint at the end of four months or four months and a half? conſequently, that the growth of the cane in ſuch a ſoil is nearly equal to what it would be either in a moiſt or in a well-manured ſoil, having a good expoſition, with this difference however, that the moiſter the ſoil is, the quicker the canes joint, and the thicker and longer the joints are: ſtill, however, all the real growth of any conſequence takes place in the ſpace of from eleven to twelve months.

Obſervation

à peu près le non plus ultra de l'accroiſſement des cannes dans un terrein bon, quoique ſec, point fumé, mais aſſez bien travaillé et aidé de la ſaiſon.

Et ne peut-on pas conclure des trois obſervations reünies, que ces 30 ou 34 nœuds ſont l'ouvrage d'autant de ſemaines écoulées depuis la ſortie du premier nœud à quatre mois quatre mois et demi, qu'ainſi l'accroiſſement des cannes dans un pareil terrein, eſt à peu près egal à celuy qu'elles prendroient dans un terrein humide ou dans un autre bien expoſé et egouté, avec cette difference cependant que les cannes nouent d'autant plus vite que le terrein eſt plus humide, et que dans ce cas les nœuds ſont plus gros et plus longs, mais qu' enfin tout l'accroiſſement réel ſe trouve renfermé dans l'eſpace d'environ douze à 13 mois.

Obſervation the 11th, In a ſoil which is drier and more parched, particularly in one where tilling or the ſeaſon have not a little made up for the diſadvantages of the expoſition and the ſoil, I have never ſeen canes with more than from twenty-four to twenty-eight joints; but this number I have ſeen even on canes which were not more than two feet high.

12thly, I have never ſeen the cane joint earlier than the end of the fifth month in this ſort of ſoil.

13th, Whenever, after having attempted to cut theſe canes in January, at fifteen months, I have waited for them till April, in conſequence of not being ſatisfied with their appearance, I have not it is true found one of them rotted, but I have found many dried up.

14th,

11e obſervation, Dans un terrein plus ſec, plus aride, et ſur tout ſi le travail ou la ſaiſon n'ont pas un peu balancé le deſavantage de l'expoſition et du ſol, je n'ai jamais vu de cannes qui euſſent au dela de 24 à 28 nœuds, et j'ai vu cette quantité de nœuds même dans des cannes qui n'avoient que deux pieds de haut.

12e obſervation, Je n'ai jamais vu dans un terrein de cette eſpéce la canne nouée avant cinq mois.

13e obſervation, Lorſqu' ayant voulu couper les cannes en Janvier à 15 mois, et n'ayant pas été ſatisfait de leur etat actuel, je les ai attendües juſqu'en Avril, je n'en ai pas trouvé à la verité une ſeule de pourrie, mais j'en ai trouvé beaucoup d'entierement deſſechées.

14th, When canes planted in the ſame kind of ſoil have been either of twelve or fifteen months growth in March or April, and that, either unwilling or unable to cut them then, I have waited till May or June, I have always found many dead ſtumps, the firſt productions entirely dried up in thoſe which had ſtood out beſt, and the ſecond ſo changed that it has been with great trouble and great attention that I have been able to make ſugar of them.

May not one infer from the eleventh, thirteenth, and fourteenth obſervations, that from twenty-four to twenty-eight joints are the greateſt growth which canes can take in ſuch a ſoil?

And may one not infer from the twelfth obſervation, that theſe joints are the produce of an equal number of weeks,

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14^e obſervation, Dans ce même terrein, lorſque les cannes ont eû dans le mois de Mars et d'Avril ſoit 12 ſoit 15 mois, et que je n'ay pas voulu ou pu les couper alors, j'ai trouvé au mois, de May et de Juin ſuivant, beaucoup de ſouches mortes, j'ai trouvé dans celles qui avoient reſiſté, le plus grand nombre des *premieres* productions totalement deſſechées, et les *ſecondes* ſi alterées par le ſoleil, que ce n'a été qu'avec beaucoup de peine et d'attention qu'on a pu en faire du ſucre.

Ne peut on pas conclure de la 11^e, 13^e, et 14^e obſervation que ces 24 à 28 nœuds des cannes dans cette eſpéce de terrein ſont le non plus ultra de l'accroiſſement qu'elles peuvent y prendre.

Et ne peut on pas conclure de la 12^e, que ces 24 à 28 nœuds ſont l'ouvrage d'autant

weeks, which being added to the five months or five months and a half, which elapſed before the appearance of the firſt joint, make only twelve or thirteen months of real growth, both for the canes which are planted in a dry ſoil, and for thoſe which are planted in a moiſt one, the difference only, as I obſerved higher, there is between the length and thickneſs of the joints excepted?

15th obſervation, When good care has been taken to manure and work theſe dry places, I have always ſeen the productions of them every way equal to thoſe which had come from a ſoil well expoſed and well drained.

16th obſervation, When heaps of dung have been laid upon particular parts of ſuch a ſoil, in order to diſtribute them thence throughout the remainder of the plantation, I have always ſeen the canes, grown in which had thoſe places which had been covered with the dung, rotted or dried.

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d'autant de ſemaines qui jointes au cinq mois, cinq mois et demi ecoulés avant la ſortie du premier nœud ne font que douze à 17 mois d'accroiſſement réel dans les cannes du terrein le plus humide, reſervé comme j'ai dit plus haut la différence de longueur et de groſſeur des nœuds.

15e obſervation, Lorſqu'on a eu ſoin de travailler parfaitement et de bien fumer ces endroits arides, j'y ai toujours vu les productions egales en tout à celles d'un bon terrein bien expoſé et egouté.

16e obſervation, Lorſqu'on a fait des tas de fumier ſur quelques parties de ce terrein ſec et avide pour les diſtribuer dans le reſte de la piéce, j'ai toujours vu les cannes dans ces petits eſpaces qui avoient été couverts de fumier, pourries

ou

dried (according to the ſeaſon) at the thirteen months end, as the canes of the moiſt ſoil were. This difference, however, I found between the two ſorts when both were cut at the twelvemonths end; the ſugar which was made of the canes that had grown on the ſoil that was too much dunged burned in April; whereas that made from the canes of the moiſt ſoil was finer then than at any other ſeaſon.

17th, I have ſeen the effects of theſe heaps of dung ten years after. The part which had been covered with it gave excellent productions at that diſtance of time.

May we not conclude from the fifteenth and ſixteenth obſervations, that it is always the fault of the planters, and never that of the plant, if it happens that the cane is not fit to cut at the twelve months end? Now, ſuppoſing

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ou deſſechées, ſuivant la ſaiſon, à 13 mois, comme les cannes du terrein humide; j'y ai cependant obſervé cette différence qu'en les coupant les unes et les autres à douze mois, le ſucre des cannes du terrein trop fumé bruloit en Avril, et que celuy des cannes du terrein humide, ſe feſoit alors plus beau qu'en aucune autre ſaiſon.

17^{e} obſervation, J'ai vu, même après dix ans, l'effet de ces tas de fumier, et tous les eſpaces ſur leſquels ils avoient été mis donner encore *d'excellentes productions*.

Ne peut-on pas conclure de la 15^{e} et 16^{e} obſervation que c'eſt toujours la faute du cultivateur et jamais celle de la plante ſi la canne n'eſt pas bonne à couper

poſing that the canes planted in October and November are fit to be cut at the end of the twelvemonth, the rains which fall at that time prevent the making any ſugar, unleſs one chuſes to make it execrable, and kill all one's cattle. I ſay nothing of December plantations, becauſe, except in very highly favoured ſoils indeed, it is rare that one plant in ten ſucceeds, the dry weather is too near.

May we not infer from the ſeventeenth obſervation, that it is likewiſe the fault of the planters, and by no means that of the ſoil, if the latter does not afford excellent productions, that is, in other words, good rattoons at the ſixth cutting?

Finally, may we not infer from the hiſtory of the cane in all kinds of ſoils, that if there be any in which it can

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couper à 12 mois; or en ſuppoſant que les cannes plantées en Octobre et Novembre fuſſent bonnes à couper douze mois après, la pluye qui tombe alors en abondance ne permettroit pas de faire du ſucre à moins qu'on ne voulut le faire abominable et ecraſer les beſtiaux. Je n'ai point parlé ici des plantations de Decembre parce qu'il eſt rare que dans les terreins qui ne ſont pas privilegiés, il en reuſſiſſe deux ou trois ſur dix, le ſec eſt trop voiſin?

Ne peut-on pas conclure de la 17^{e} obſervation que c'eſt auſſi la faute du cultivateur, et point du tout celle du terrein, s'il ne donne pas encore *d'excellentes productions*, c'eſt à dire en d'autres mots, de bons rejettons, à la ſixieme coupe.

Et ne peut-on pas enfin conclure de l'hiſtoire générale de la canne dans toutes les eſpéces de terreins, que s'il en eſt, ou elle peut ſubſiſter juſqu'à 15 et 16 mois,

can exiſt till the fifteenth or ſixteenth month, it never grows to any kind of purpoſe in any after the thirteenth. Perhaps the accuracy with which I have followed it may authorize me to ſay boldly after the twelfth.

Hiſtory of a ſingular revolution in the inſide of the cane, and of the arrow which comes out in conſequence of that revolution, and conſtitutes the laſt ſtage of the plant's exiſtence.

A principal and neceſſary effect of the dry weather on the cane is, the diminution of the watery parts of its juice. There is no reaſon, therefore, for being ſurprized, either that the ſame quantity of canes which affords from 140 to 160 gallons of it in January ſhould only afford 90 to 100 in April, or that there ſhould be as much

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mois, elle ne croît cependant jamais d'une façon utile au dela de 13 ? peut être même dirai-je affirmativement douze, après l'avoir ſuivie juſques dans ſon dernier developpement.

Hiſtoire d'une revolution ſinguliere dans l'interieur de la plante ; de la fleche qui la ſuit, et qui forme le dernier developpement de la canne.

L'effet principal et neceſſaire du ſec ſur la canne, eſt la diminution de la partie aqueuſe de ſon jus ; l'on ne ſera donc point ſurpris que la même quantité de cannes qui en donne au mois de Janvier 140 à 160 gallons, n'en donne que 90 à 100

much ſugar made from the latter as from the former. This follows of courſe; but it ſhould ſeem, that upon the return of the rains, the cane ſhould recover the water, which the heats of February, March, and April, had made it loſe: the reverſe, however, is what in fact happens; not only the quantity of the juice is leſs, but the quality of it is worſe; it contains leſs ſugar in it, and when the weather is fairly ſettled, when the rains are frequent and abundant, which generally happens from the firſt of July to the 13th of Auguſt, the whole vegetative faculties of the plant ſeem employed in the production of the arrow, ſee *oop*, fig. 1. During that time the body of the cane is almoſt totally deſtitute of juice, which is carried in great plenty towards the head of it. Soon after there comes out from the top of the arrow, a thin

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à 100 dans le mois d'Avril, n'y que ces 90 gallons d'Avril donnent autant de ſucre que les 160 de Janvier: mais il ſembleroit qu'au retour des pluyes, la canne devroit recouvrer l'eau qu'elle a perdue par le ſoleil conſtant de Fevrier, Mars, et Avril; c'eſt le contraire qui arrive; non ſeulement le jus diminüe, mais ſa qualité ſe deteriore, il contient moins de ſucre, et lorſque le renouveau eſt tout à fait decidé, que les pluyes ſont frequentes et abondantes, ce qui arrive ordinairement du 1 Juillet au 15 d'Aouſt, il ſemble que tous les efforts de la vegetation ſoient conſacrés à la production de la fleche, v. *oop*, fig. 1. Le corps de la canne eſt alors preſque totalement deſtitué de ſon jus qui ſe porte en abondance vers la tête de la plante; bientôt il ſort de ſon extremité une tige

thin pointed ſtem, which grows about three feet high in the ſpace of five or ſix weeks, then blows at the end, and is crowned with a panicle like that of a reed. *oop*, fig. 1. ſhews the arrow crowned with its panicle; it is commonly full-blown towards the end of October, withers ſoon after, and falls in the following month. Some people pretend, that this panicle contains the ſeed of the cane; I have ſown the kind of ſeed or duſt which comes from it, and nothing has come up. Perhaps the cane which is propagated by ſhoots bears only empty huſks. Be this as it may, when once the arrow has appeared, the cane is in its laſt ſtage.

After the fall of the leaf, which takes place a little ſooner or a little later, according to the ſeaſon, the joints which preceded it dry up, in their order. This mortification,

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aſſez grêle et pointue qui s'eleve dans l'eſpace de cinq à ſix ſemaines juſqu'à la hauteur d'environ trois pieds, ſe developpe enfin par ſa pointe et ſe couronne d'un panache ſemblable à celuy du roſeau. *oop*, fig. 1. preſente la fleche couronnée de ſon panache; elle eſt ordinairement tout à fait developpée à la fin d'Octobre, peu après elle ſe deſſeche, elle tombe le mois ſuivant. Quelques perſonnes pretendent que ce panache contient la ſemence de la canne, je n'en ſçais rien, mais j'ai ſemé l'eſpece de pouſſiere ou de graine qui en ſort, et rien n'a levé, peut-être que la canne qui vient de bouture ne porte que des graines folles. Quoiqu'il en ſoit, après que la fleche eſt ſortie, la canne eſt à ſon dernier periode; et un peu plus tôt, un peu plus tard, après la chute de la feuille (toujours ſuivant la ſaiſon) les nœuds qui la precedent ſe deſſechent ſucceſſivement, et cette

tification, if I may ſo call it, never ſtops, except when there happens to come out a kind of ſpurious top about the eighth or ninth joint from the arrow, ſee *l*, *m*, fig. 1.

It comes out of two joints which blow after the fall of the arrow, when there happens to be much rain. This ſpurious head ſerves much the ſame purpoſes as the true one; for it aſſiſts, maintains, and preſerves the action of the ſap in that part of the cane which ſtill remains alive, ſo that whenever there is not rain enough to produce it, the laſt joints die off like the former.

It is farther to be obſerved with regard to the arrow, 1ſt, that this any more than the ripeneſs does not depend upon the age of the cane, but upon the ſeaſon. In October and November the canes of ſix months growth bear arrows like thoſe of twelve or fifteen months, but all canes do not bear arrows.

2d, Though

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cette gangrene, ſi j'oſe m'expliquer ainſi, n'eſt arrétée que lorſqu'il ſe forme une eſpéce de fauſſe tête vers le 8e et 9e avant dernier nœud, v. *l*, *m*, fig. 1.

Ce ſont deux nœuds qui ſe developpent après la chute de la fleche, s'il y a beaucoup de pluye, et qui forment cette fauſſe tête qui fait en partie l'office de la premiere, puis qu'elle entretient l'action de la ſeve dans la partie de la canne qui reſte encore ſaine, et que s'il ne fait pas aſſez de pluye pour que ces deux nœuds ſe developpent, tous ſe deſſechent ſucceſſivement juſqu'à la racine.

Obſervons encore par rapport à la fléche, 1°, qu'ainſi que la maturité, elle n'eſt point l'effet de l'age de la canne, mais de la ſaiſon; dans le mois de Septembre ou d'Octobre les cannes de ſix mois *flechent* comme celles de 12 et de 15; mais toutes les cannes ne ſont pas ſuſceptibles de ce phenomene.

2d, Though ſome canes have no arrow, they are all ſubject to that internal revolution of which I have ſpoken, *viz.* the diminution and impoveriſhment of their juices.

3d, Whenever in a plantation, the ſoil of which, though good, had been accidentally abandoned, there has happened to be found a ſhrub ſtrong enough to ſerve as a prop to a cane having an arrow and ſhoots from its eighth or ninth joint (*l*, *m*, fig. 1.); theſe ſhoots have each of them produced a cane: far inferior indeed in beauty to that out of which they had themſelves ſprung, but which, doubtleſs, would have wanted nothing to have born an arrow and canes in their ſeaſon alſo, but ſuch another ſhrub as that to which they owed their exiſtence.

I know nothing of the obſervations upon which the opinion is grounded, that the cane does not come to per-

fection

2°, Que les cannes qui ne flechent pas n'en éprouvent pas moins la revolution interieure dont j'ay parlé, cette diminution et appauvriſſement de leur jus.

3°, Que lorſqu'il s'eſt trouvé dans une piéce de cannes abandonnée (bonne terre cependant) quelque arbriſſeau qui a pu ſervir d'appuy à une canne flechée et aux jets ſortis de ſon 8e et 9e nœud (*l*, *m*, fig. 1.). Ces jets ont donné chacun une canne beaucoup moins belle à la verité que celle dont ils ſortoient, mais qui n'auroit ſans doute eu beſoin pour flecher dans ſon tems, et donner enſuite d'autres petites cannes, que d'être appuyée comme celle qui l'avoit produite.

J'ignore les obſervations d'après leſquelles on peut prétendre que la canne a

fection under fourteen or fifteen months. Thoſe of which I have been giving an account ſeem very poſitively to eſtabliſh that, *conſidered with regard to its greateſt uſefulneſs, the cane is an annual plant producing by the root*; conſequently, the mode of cultivation which agrees beſt with it, that which will anſwer beſt to the planter, will be that which, conſidering theſe as the two capital points to be attended to, connects them the beſt with the neceſſary influence the ſeaſons of the climate in which the cane grows will have upon it. Now I truſt that in theſe two reſpects my method will ſtand the teſt of examination.

It remains to ſpeak of the hiſtory of the cane according to the two methods.

Hiſtory

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beſoin de 14 à 15 mois pour être dans ſa perfection: celles que je viens de preſenter me paroiſſent établir d'une façon bien poſitive que *relativement à ſon plus grand degré d'utilité la canne eſt une plante annuelle, et vivace par ſa racine,* qu'ainſi le ſyſteme de culture qui lui convient, le ſyſteme le plus avantageux au cultivateur ſera celuy qui aura ces deux points pour baſe, et qui les combinera le mieux avec les effets que doivent operer ſur elle, les différentes ſaiſons qui regnent dans les climats où on la cultive, et j'oſe croire que mon ſyſteme, peut ſoutenir l'examen à ces deux egards.

Il me reſte à parler de l'hiſtoire de la canne dans les deux ſyſtemes.

Hiſtoire

History of the cane according to the two different methods of cultivation, and in different years, the favourable, the dry, and the rainy.

The hiſtory of the cane, according to both methods, flows ſo naturally from what I have ſaid above, as well of the ſeaſons as of their influence on the different productions, firſt, ſecond, and third, that the bare recollection of the time and conditions of their appearance and developement will be ſufficient to lead us to conclude,

1ſt, That in favorable ſeaſons, the *firſt* productions of canes planted in October and November, and cut fifteen months after, have a greater number of joints than thoſe of canes planted in May, and cut the year after at the end of

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Hiſtoire de la canne dans les deux ſyſtemes, et dans les différentes eſpéces d'années, favorables, ſeches et pluvieuſes.

L'hiſtoire de la canne dans les deux ſyſtemes découle ſi neceſſairement de ce que j'ai dit cy deſſus tant à l'egard des ſaiſons, que de leur influence ſur les différentes productions *premieres*, *ſecondes*, et *troiſiemes*, qu'il ſuffit de ſe rappeller le tems et les conditions de leur ſortie et de leurs developpements, pour conclure:

1°, Que dans les années favorables, les *premieres* productions des cannes plantées en Octobre et Novembre, coupées quinze mois après ont plus de nœuds que les *premieres* productions des cannes plantées en May, et coupées l'année d'après

of twelve months; but that this exceſs is only equivalent to the vegetation of one month, on account of the dry weather, which neceſſarily impedes it during the months of March and April. As to the *ſecond* productions, they are equal in the two methods, thoſe upon the larger ſcale of cultivation appearing in June to be cut in February, and thoſe of the ſmaller appearing at the end of Auguſt to be cut in May.

2d, The loſs in years of drought muſt be a little more conſiderable on the ſmall plan than on the other; becauſe there being no *ſecond* productions (of real value) in that caſe according to either of the plans, and the difference of the thirteenth month, with regard to the *firſt*, remaining entire, one on thirteen muſt of courſe be more felt than one on a greater number.

3d, In

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d'après à 12 mois, mais que ce plus n'equivaut qu'à un mois de vegetation, attendu l'effet du ſec qui dans une terre decouverte, la ſuſpend viſiblement pendant les mois de Mars et d'Avril; et que les *ſecondes* productions ſont egales dans les deux ſyſtemes; les *ſecondes* productions de la grande culture ne paroiſſant qu'en Juin pour être coupées en Fevrier, et les *ſecondes* productions de la petite culture paroiſſant à la fin d'Aouſt pour être coupées en May.

2°, Que dans les années ſeches, la perte doit être un peu plus ſenſible dans le ſyſteme de la petite culture, parce que dans l'un n'y dans l'autre ſyſteme on ne verra paroître de ſecondes productions (d'une valeur réelle) et que la difference du treizieme mois ſur les *premieres* demeurant en ſon entier, un eſt plus ſenſible ſur treize que ſur un nombre plus conſiderable.

3°, Que

3d, In rainy years the *ſecond* and *third* productions will be the ſame, according to both the methods, on account of the reaſon given above; to wit, the equality of the time which elapſes between their firſt appearance and the time of their being cut; and becauſe the advantage which the great method might expect from its *firſt* productions will be loſt by their being rotten at the time of cutting.

Finally, that the total difference in the produce of a number of years taken together, cannot be conſiderable enough not to be much more than made up for by the produce of my ſecond crop, or rather by cutting the whole land, inſtead of cutting only three parts of it.

Notwithſtanding the ſtrong reaſons I have to determine me to the choice of the ſmall plan, I confeſs the force

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3°, Que dans les années pluvieuſes, les *ſecondes* et *troiſiemes* productions ſeront egales dans les deux ſyſtemes par la raiſon que nous avons donnée cy deſſus de l'egalité du tems qui s'ecoule entre la ſortie et la coupe des unes comme des autres, et que le benefice que le ſyſteme de la grande culture pourroit eſpérer de ſes *premieres* productions ſe trouvera en pourriture au tems de la coupe.

4°, Enfin que la différence totale ſur le produit des années combinées ne peut être aſſez conſiderable pour n'être pas beaucoup plus que compenſée par le produit de ma ſeconde recolte, ou plutôt par une recolte ſur toute la terre au lieu d'une recolte ſur les trois quarts.

Malgré l'evidence des raiſons qui devoient me decider à ſuivre le ſyſteme de la petite culture j'avoue que la force du vieux prejugé m'a tenu moy même très long

force of old prejudices long held me in ſuſpence. I think, however, I may with confidence propoſe it, as I have the experience of five years to bear me up in aſſerting that, one year with another, there is a difference of above one ſixth in its favour.

EXPLA-

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long tems en ſuſpens, mais je crois devoir le propoſer lorſque je puis prouver par une expérience de cinq années conſecutives une différence de quelque choſe de plus qu'un ſixieme en ſa faveur, une année priſe dans l'autre.

EXPLANATION OF THE FIGURES.

Figure the firſt ſhews a cane entirely without leaves.

N° 1. Is the firſt order of roots; they come out of the joint to which the bud which is to produce the cane is faſtened. All they ſtand in need of to make them come out is the moiſture which is contained in the medullar part of the plant, and the little fermentation which ariſes from the want of circulation after the two extremities *y* and *z* have been cut.

N° 2. Shews the ſecond order of roots, compoſed of its firſt ſet *aa*, of its ſecond *bb*, of its third *cc*, of its fourth *dd*. Theſe four ſets of the ſecond order come out

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La figure entiere repreſente une canne à ſucre dépouillée entierement de ſes feuilles.

N° 1. Preſente le premier ordre de racines, elles ſortent du nœud auquel eſt attaché le bouton qui doit produire la canne. Elles n'ont beſoin pour ſortir que de l'humidité contenue dans la partie médullaire du plan et de la petite fermentation qui ſuit le défaut de circulation ou de balancement de la ſéve après que les deux extrémités *y* et *z* ont été coupées.

N° 2. Preſente le ſecond ordre de racines compoſé de ſon premier plan *aa*, du ſecond *bb*, du troiſieme *cc*, du quatrieme *dd*; ces quatre plans du ſecond ordre

out of the quadruple ſheath *abcd*, which contained the bud. The firſt ſet of theſe roots cannot come out without puſhing away the leaf which wraps up the whole, no more can any of the reſt; I believe the part marked *r* is the neck of the plant, or the place at which it comes out of the ground.

N° 3. Shews the third order of roots, compoſed of its firſt ſet *ee*, of its ſecond *ff*, of its third *gg*, and of its fourth *hh*. Each of theſe ſets as it comes out likewiſe throws down one of thoſe leaves, which in the body of the work I have called the external, or *proper* leaves of the plant. What is meant to be ſhewn here is rather the place than the length of them, as they are commonly between twenty and thirty inches long. *x* and *x* are the ſecond productions or immediate growths of the ſtump.

Theſe

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ordre proviennent du quadruple étui de feuilles ſeminales *abcd*, qui enveloppoient l'embryon. Le premier plan de ces racines n'a pu ſortir ſans chaſſer la feuille enveloppe du tout; ainſi des autres. Je crois que la partie *r* forme le collet de la plante.

N° 3. Preſente le troiſieme ordre de cannes compoſé de ſon premier plan *ee*, du ſecond *ff*, du troiſieme *gg*, du quatrieme *hh*: chacun de ces plans fait ainſi tomber en ſortant une de ces feuilles que j'appelle dans l'extrait, extérieures ou propres de la plante: je déſigne ici plutôt leur place que leur longueur puiſqu'elles ont ordinairement depuis vingt juſqu'à trente pouces. *x* et *x* ſont les

Theſe ſecond productions come from buds which adhere to ſets of roots of the third order, which third order is the only one which gives productions out of the earth. The third productions, *t*, *t*, come out of the ſecond. The number of the ſets of roots of the third order is unlimited. The ſecond is confined to four ſets. The part between *i* and *k* ſhews the effects of a very dry ſeaſon; the joints are ſtunted, and ſometimes only two lines long, inſtead of an inch and a half or two inches. When the rains begin again, the joints which come out are again larger; ſee from *k* to *l*, where they are as big as they were between *h* and *i*.

The part from *l* to *n* is what is made uſe of for planting; *yz* is the plant without its leaves. It is, however, commonly put into the ground with its leaves, as in

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ſecondes productions propres de la ſouche, ces ſecondes productions ſortent des boutons attachés aux plans de racine du troiſieme ordre qui ſeul donne des productions hors de terre. Les troiſiemes productions *t* et *t* ſortent des ſecondes. Le nombre des plans de racine du troiſieme ordre eſt illimité; le ſecond ordre eſt borné à quatre plans. Depuis *i* juſques à *k* on voit l'effet d'un ſec très vif, les nœuds ſont étranglés et n'ont quelquefois que deux lignes au lieu d'un pouce et demi deux pouces. Lorſque les pluies recommencent les nœuds qui ſortent ſont plus longs, comme on les voit depuis *k* juſqu'à *l*, tels qu'ils étoient depuis *h* juſqu'à *i*.

La partie depuis *l* juſqu'à *n* eſt celle qui ſert de plan; *yz* eſt le plan dépouillé de ſes feuilles, on le met cependant en terre avec ſes feuilles tel qu'il

in figure 7th. *u* is the ligament by which the bud is faſtened to the plant. *oo* is the laſt production of the plant, and is called the arrow; it appears at firſt as an arrow (*o o*), and is then crowned with a pannicle *p*; it ſhoots in September or October, and falls in November or December. It is ſaid, that this pannicle contains the ſeed of the plant.

I have ſown it, but nothing has come up. When the arrow has dried, and is fallen off, the joints below it dry one after the other, when there is little or no rain; but if there happens to be much rain, the eighth and ninth, or tenth and eleventh joints from the top (ſee from *l* to *m*) unfold, and form a ſecond head, which keeps up the vegetation in the lower part of the plant ſome time longer. If theſe joints were to be ſupported

as

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qu'il eſt dans la figure 7. *u* eſt l'ombilic par lequel l'embryon eſt attaché au plan. *oo* eſt la derniere production de la canne: on l'appelle la fleche: elle paroit d'abord comme une fleche telle qu'elle eſt en *oo* et ſe couronne enſuite d'une panache *p*, elle ſort en Septembre ou Octobre, et tombe en Novembre ou Decembre. On pretend que ce panache contient la graine de la plante.

J'ai ſemé cette prétendue graine, rien à levé. Après que la fleche eſt ſéchee et tombée, les nœuds qui la prêcedent ſe deſſechent ſucceſſivement, s'il ne pleut que très peu, ou bien les huitieme et neuvieme, ou dixieme et onzieme avant dernier nœud *lm* ſe développent et forment une ſeconde tête qui entretient encore la vegetation dans le reſte de la plante. Ces nœuds appuyés à meſure qu'ils

ſe

as they come out, they would produce canes, but of an inferior ſort to the others. All canes have not arrows, and the coming out of an arrow depends on the ſeaſon, and not on the age of the cane.

ſe développent donneroient des cannes auſſi mais beaucoup moins belles que les autres ; toutes les cannes ne flechent point; et c'eſt la ſaiſon et non pas l'age de la canne qui décide la ſortie de la fleche.

XX. *Account of the* Free Martin.

By Mr. John Hunter, *F. R. S.*

Read February 25, 1779.

GENERATION, when produced from a ſeed, has two cauſes which concur towards its perfection; the one which forms the ſeed, the other which gives it the principle of action [a].

The cauſe which forms the ſeed is called the female, the other cauſe is called the male; but thoſe two cauſes in general make only a part of a whole animal, or are

(a) It may be neceſſary for ſome of my readers to have explained to them what I mean by a ſeed. I do ſuppoſe, that the word ſeed was firſt applied to grain, or that which is always called ſeed in the vegetable; which ſeed is the part of that claſs of vegetables in which the matter of the young vegetable exiſts, or is formed. The principle of arrangement fitting the parts for action in this claſs of ſeed being at firſt not known, a falſe analogy between the vegetable and animal was eſtabliſhed, *viz.* the ſecretion of the *teſtes* (the only known principle in the animal) was called the ſeed: but from the knowledge of the diſtinct ſexes in the vegetable it is well known, that the ſeed is the female production in them, and that the principle of arrangement for action is from the male. The ſame operation and principles take place in many orders of animals, *viz.* the female produces a ſeed, in which is the matter fitted for the firſt arrangement of the organs of the animal, and which receives the principle of arrangement fitting them for action from the male.

rather

rather parts ſuperadded to an animal. Probably they were firſt conſidered in thoſe animals where thoſe parts were ſeparated, or in which the female parts were wholly found in one animal and the male in the other; therefore the terms female and male have been applied to the whole animal, dividing them into two diſtinct ſexes, and the parts which formed either the one ſex or the other called either the female or the male parts of generation; but upon a further knowledge of animals, and of thoſe parts, they were found to be united in the ſame animal in many of the inferior tribes, who, from poſſeſſing both parts, have got the name of hermaphrodite.

As both thoſe parts are natural to moſt animals, and as the union of them in the ſame animal is alſo natural to many, and the ſeparation of them in diſtinct animals, is only a circumſtance making no eſſential difference in the parts themſelves; it becomes no great effort or uncommon play in nature to unite them in thoſe animals in which they are commonly ſeparated.

And accordingly we find many of thoſe orders of animals, which have them ſeparate naturally, have them ſometimes united.

From this account hermaphrodites may be divided into two kinds; the natural, and the unnatural uncommon or monſtrous.

The natural belongs to the inferior and more ſimple order of animals, of which there are a much greater number than of the more perfect; but as animals become more complicated, have more parts, and each part is more confined to its particular uſe, a ſeparation of the two neceſſary powers for generation have alſo taken place in them.

The unnatural, I believe, now and then takes place in every tribe of animals having diſtinct ſexes, but is more common in ſome than in others (b). I fancy the human has the feweſt, never having ſeen them in that ſpecies nor in dogs: cats we know leſs of; but in the horſe, aſs, ſheep, and cattle, they are very frequent.

Though this ſpecies of hermaphrodite be a mixture of both ſexes, and ſo poſſeſſes the parts peculiar to each in perfection, there is yet one part of each which it does not poſſeſs: I mean the part which is common to both. For as this common part is different in one ſex from what it is in the other, and it is impoſſible for one animal to have both kinds; that which they do have muſt of courſe partake of both ſexes, and conſequently render the hermaphrodite imperfect *quoad hoc*.

(b) *Quere*, Is there ever in the tribe of animals, that are natural hermaphrodites, a ſeparation of the two parts?

This one or common part is the *clytoris* in the female, and *penis* in the male; and the great difference in this part between the one ſex and the other is ſize and perforation for the *ſemen*.

But thoſe parts, which are peculiar to each ſex, may be all perfectly joined in the ſame animal, which would come up to the idea of the trueſt hermaphrodite.

The hermaphrodites of this kind, which I have ſeen, have always appeared externally, and, at firſt view, to be females: and in thoſe ſpecies of animals where only the female is preſerved for breeding, as in ſheep, goats, pigs, &c. they are generally ſaved as females.

In the horſe they are very frequent: I have ſeen ſeveral, but never diſſected any. The moſt perfect I have ſeen in this ſpecies were thoſe in which the teſticles had come down out of the *abdomen* into the place where the udder ſhould have been (*viz.* more forward than the *ſcrotum*) and appeared like an udder, not ſo pendulous as what the *ſcrotum* is in the true male of ſuch animals. There were alſo two nipples, which horſes have no perfect form of, being blended in them with the ſheath or *prepuce*, of which there was none here.

The external female parts were exactly ſimilar to thoſe of the perfect female; and, inſtead of a common-

ſized *clytoris*, there was one about five or ſix inches long, which, when erect, ſtood almoſt directly backwards.

I procured a foal aſs, very ſimilar in external appearance to the above horſe, and killed it, to examine the parts. It had two nipples, but the teſticles were not come down as in the above; owing, perhaps, to the animal's being yet too young.

There was no *penis* paſſing round the *pubis* to the belly as in the perfect male aſs.

The external female parts were ſimilar to thoſe of the ſhe-aſs. Within the entrance of the *vagina* was placed the *clytoris*, but much longer than that of a true female, being about five inches long. The *vagina* was open a little further than the opening of the *urethra* into it, and then became obliterated; from thence up to the *fundus* of the *uterus* there was no canal.

At the *fundus* of the common *uterus* it was hollow, or had a cavity in it, and then divided into two, *viz.* a right and a left, called the horns of the *uterus*, which were alſo pervious.

Beyond the termination of the two horns were placed the *ovaria* as in the true female, but I could not find the *fallopian* tubes.

From the broad ligaments to the edges of which the horns of the *uterus* and the *ovaria* were attached, there

paſſed towards each groin a part ſimilar to the round ligaments in the female, which were continued into the rings of the *abdominal* muſcles; but with this difference, that there were continued with them a *proceſs* or *theca* of the *peritoneum*, ſimilar to the *tunica vaginalis communis* in the male aſs, and in theſe *thecæ* were found the teſticles: but I could not obſerve any *vaſa deferentia* paſſing from them.

Here then we found in the ſame animal the parts peculiar to each ſex (although very imperfect), and that part which is common to both (but different in each) was a kind of medium of that difference.

Something ſimilar to the above I have ſeen in ſheep, goats, &c.; but I ſhall not at preſent trouble the Society with a deſcription of hermaphrodites in general, as it is a very extenſive ſubject, admitting of great variety, which would make it appear a production of chance, whereas the intention of this paper is to ſhow a circumſtance which takes place in the production of hermaphrodites in cattle, and which appearing to be an eſtabliſhed principle in the œconomy of propagation of that ſpecies of animal, and not a production of chance, is, perhaps, peculiar to them, and, probably, the only way in which they are ever produced in this ſpecies.

It is a known fact, and, I believe, is underſtood to be univerſal, that when a cow brings forth two calves, and that one of them is a bull-calf, and the other a cow to appearance, the cow-calf is unfit for propagation; but the bull-calf becomes a very proper bull. They are known not to breed: they do not even ſhew the leaſt inclination for the bull, nor does the bull ever take the leaſt notice of them (c).

This cow-calf is called in this country a *free martin*; and this ſingularity is juſt as well known among the farmers as either cow or bull.

This calf has all the external marks of a cow-calf ſimilar to what was mentioned in the unnatural hermaphrodite, *viz.* the teats and the external female parts, called by farmers the bearing.

When they are preſerved it is not for propagation, but for all the purpoſes of an ox or ſpayed heifer, *viz.* to yoke with the oxen, and to fatten for the table (d).

They reſemble in form thoſe imperfect animals very much, *viz.* they are much larger than either the bull or the cow, and the horns grow larger, being very ſimilar to the horns of an ox.

(c) I need hardly obſerve here, that if a cow has twins, and that they are both bull-calves, that they are in every reſpect perfect bulls; or if they are both cow-calves, that they are perfect cows.

(d) *Vide* LESLIE on Huſbandry, p. 98, 99.

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The bellow of the free martin is ſimilar to that of an ox, which is not at all like that of a bull; it is more of the cow, although not exactly that.

The meat is alſo ſimilar to that of the ox or ſpayed heifer, *viz.* much finer in the fibre than either the bull or cow; and they are more ſuſceptible of growing fat with good food. By ſome they are ſuppoſed to exceed the ox and heifer in delicacy of food, and bear a higher price at market.

However, it ſeems that this is not univerſal; for I was lately informed by CHARLES PALMER, eſq. of Luckley in Berkſhire, that there was a free martin killed in his neighbourhood, and, from the general idea of its being better meat than common, every neighbour beſpoke a piece, which turned out nearly as bad as bull beef, at leaſt worſe than that of a cow. It is probable, that this might ariſe from this one having more the properties of the bull than the cow, as we ſhall ſee hereafter that they are ſometimes more the one than the other[e].

Free

(e) The Romans called the bull *taurus:* they, however, talked of *tauræ* in the feminine gender. And STEPHENS obſerves, that it was thought the Romans meant by *tauræ*, barren cows, and called them by this name becauſe they did not conceive any more than bulls. He alſo quotes a paſſage from COLUMELLA, lib. vi. cap. 22. "and like the *tauræ*, which occupy the place of "fertile

Free martins are ſaid to be in ſheep[f]; but from the accounts given of them, I ſhould very much ſuſpect that theſe are hermaphrodites produced in the common way, and not like thoſe of cattle. They are often imperfect males, ſeveral of which I have ſeen. They are mentioned as both male and female, which is not reconcileable to the account given of the free martin.

I believe it has never been even ſuppoſed what this animal is, with all thoſe peculiarities.

From the ſingularity of the animal, and the account of its production, I was almoſt ready to ſuppoſe the account a vulgar error; yet from the univerſality of its teſtimony it appeared to have ſome foundation; and therefore I made all the inquiry I could for an opportunity of ſeeing one, and alſo to examine it. Since which time I have accordingly had an opportunity of ſeeing three; the firſt of which was one belonging to JOHN ARBUTHNOT, eſq. of Mitcham, which was calved in his own farm. He was ſo obliging as to give me an opportunity of ſatisfying myſelf. He allowed me, firſt, to have a drawing made of the animal while alive, which was exe-

" fertile cows, ſhould be rejected, or ſent away." He likewiſe quotes VARRO, *De re Ruſtica*, lib. ii. cap. 5. "The cow which is barren, is called *taura*." From which we may reaſonably conjecture, that the Romans had not the idea of the circumſtances of their production.

(f) LESLIE's Huſbandry, p. 156.

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cuted by Mr. GILPIN. When the drawing was made of Mr. ARBUTHNOT's free martin, JOHN WELLS, eſq. of Bickley Farm, near Bromley in Kent, was preſent, and informed us, that a cow of his had calved two calves; and that one was a bull-calf, and the other a cow-calf. I deſired Mr. ARBUTHNOT to ſpeak to Mr. WELLS to keep them, or let me buy them of him; but, from his great deſire for natural knowledge, he very readily preſerved both, till the bull ſhewed all the ſigns of a good bull, when he ſold him.

From the diſſection of the three above mentioned free martins it plainly appeared, that they were all hermaphrodites differing from one another; as is alſo the caſe in hermaphrodites in other tribes.

The Deſcription of the three Free Martins.

Mr. ARBUTHNOT's Free Martin (g).

The external parts were rather ſmaller than in the cow. The *vagina* paſſed on, as in the cow, to the opening of the *urethra*, and then it began to contract into a ſmall canal, which paſſed on to the diviſion of the *uterus* into the two horns, each horn paſſed along the edge of the broad ligament laterally towards the *ovaria*.

At the termination of thoſe horns were placed both the *ovaria* and the teſticles; both were nearly of the ſame ſize, which was about as large as a ſmall nutmeg.

To the *ovaria* I could not find any *Fallopian* tube.

To the teſticles were *vaſa deferentia*, but they were imperfect. The left one did not come near the teſticle; the right only came cloſe to it, but did not terminate in a body called the *epididymis*. They were both pervious, and opened into the *vagina* near the opening of the *urethra*.

(g) This animal was about ſeven years old, had been often yoked with the oxen; at other times went with the cows and bull, but never ſhewed any deſires for either the one or the other.

On the poſterior ſurface of the bladder, or between the *uterus* and bladder, were the two bags, called *veſiculæ ſeminales* in the male, but much ſmaller than what they are in the bull: the ducts opened along with the *vaſa deferentia*. This was more deſerving the name of hermaphrodite than the two following; for it had a mixture of all the parts, although all were imperfect.

Mr. WRIGHT'S Free Martin, *five years old.*

The *vagina* terminated in a blind end, a little way beyond the opening of the *urethra*, beyond which the *vagina* and *uterus* were impervious. The *uterus* at its extreme part divided into two horns. At the termination of the horns were placed the teſticles inſtead of the *ovaria*, as is the caſe in the female. The reaſons why I call thoſe bodies teſticles are the following. Firſt, they were more than twenty times larger than the *ovaria* of the cow, and nearly as large as the teſticles of the bull, particularly as thoſe of the ridgill, the bull whoſe teſticles never come down. Secondly the ſpermatic arteries were exactly ſimilar to thoſe of the bull, eſpecially of the ridgill. Thirdly, the *cremaſter* muſcle paſſed up from the

the rings of the abdominal muſcles to the teſticles, as it does in the ridgill [b].

There were the two bags placed behind the bladder, between it and the *uterus*. Their ducts opened into the *vagina*, a very little way beyond the opening of the *urethra*; but there was nothing ſimilar to the *vaſa deferentia*.

As the external parts had more of the cow than the bull, the *clytoris*, which may alſo be reckoned an external part, was alſo ſimilar to that of the cow; not at all in a middle ſtate between the *penis* of the bull and the *clytoris* of the cow, as I have deſcribed in the hermaphrodite horſe. There were four teats; the glandular part of the udder was but ſmall.

This animal cannot be ſaid to have been a mixture of all the parts of both ſexes, for the *clytoris* had nothing ſimilar to the *penis* in the male, and was different in the cow part, in having nothing ſimilar to the *ovaria*, nor was the *uterus* a cavity.

(b) Although I call theſe bodies teſticles for the reaſon given, yet they were only imitations of ſuch, for when cut into they had nothing of the ſtructure of the teſticle: not being ſimilar to any thing in nature, they had more the appearance of diſeaſe. From the ſeeming imperfection of the animal itſelf, it was not to be ſuppoſed that they ſhould be teſticles, for then the animal ſhould have partook of the bull, which it certainly did not.

Mr. WELLS's Free Martin.

This animal was never ſeen to ſhew any ſigns of a deſire for the male, although it went conſtantly with one. It looked more like an heifer than what they commonly do; but as it was only between three and four years old when killed, it is very probable, that it was not ſufficiently old to have taken the characters of the ox; however, this may be owing to another circumſtance that will be mentioned hereafter.

The teats and udder were ſmall compared with thoſe of a heifer, but rather larger than in either of the former; the beginning of the *vagina* ſimilar to that of the cow, but it ſoon became obliterated beyond the opening of the *urethra*, as in the laſt deſcribed. The *vagina* and *uterus* to external appearance was continued, although not pervious, and the uterine part divided into two horns, at the end of which were the *ovaria*.

I could not obſerve in this any other body which I might have ſuppoſed to be the teſticle.

There was on the ſide of the *uterus* an interrupted *vas deferens* broken off in ſeveral places.

Behind the bladder, or between it and the *vagina*, were the bags called *veſiculæ ſeminales*, between which were the terminations of the two *vaſa deferentia*.

The ducts of the bags and the *vaſa deferentia* opened as in the former.

This could not be called an exact mixture of all the parts of both ſexes, for here was no appearance of teſticles.

The female parts were imperfect, and there was the addition of part of the *vaſa deferentia*, and the bags called *veſiculæ ſeminales*.

This circumſtance of having no teſticles, perhaps, was the reaſon why it had more the external appearance of a heifer than what they commonly have, and more than either of the two former had.

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THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January 1778.

	Time. H.	M.	Therm. without	Therm within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Jan. 1	8	0	34,0	35,0	29,69		N	1	Cloudy.
	2	0	35,0	35,0	29,92		N	1	Cloudy.
2	8	0	32,5	34,5	29,95		N by W	1	Cloudy.
	2	0	36,0	35,5	29,94		NW	1	Fair.
3	8	0	27,0	32,0	29,94		SE	1	Fair and frosty.
	2	0	36,0	34,0	29,84		SE	1	Fair and frosty.
4	8	0	37,0	35,5	29,72	0,169	W by N	1	Cloudy.
	2	0	37,0	37,5	29,77		WNW	1	Foggy.
5	8	0	35,5	37,0	29,80		ENE	1	Cloudy.
	2	0	36,0	37,5	29,91		NE	1	Cloudy.
6	8	0	35,5	35,5	30,31		SE	1	Cloudy.
	2	0	37,0	37,5	30,31		NE	1	Fair.
7	8	0	34,0	37,5	30,28		SW	1	Cloudy.
	2	0	34,0	39,0	30,26		SW	1	Cloudy.
8	8	0	32,0	35,5	30,26		NE	1	Cloudy.
	2	0	35,0	35,5	30,27		NE	1	Fair.
9	8	0	32,5	35,0	30,32		NE	1	Fair.
	2	0	33,0	34,5	30,30		NE	1	Fair.
10	8	0	35,0	35,5	30,16		ENE	1	Fair.
	2	0	37,0	36,5	30,07		NW	1	Fair.
11	8	0	32,5	35,0	29,90		NE	1	Fair.
	2	0	35,5	36,0	29,92		NE	1	Snow.
12	8	0	33,0	35,0	29,63		ESE	1	Snow.
	2	0	38,0	37,5	29,57		ESE	1	Fine.
13	8	0	33,5	36,0	29,43	0,222	SE	1	Snow.
	2	0	35,5	38,5	29,35		SE	1	Fair.
14	8	0	33,5	37,0	28,87	0,095	E by S	2	Cloudy.
	2	0	37,0	39,0	28,59		ESE	1	Rain.
15	8	0	40,0	41,0	28,68	0,139	S by E	1	Fair.
	2	0	45,0	42,5	28,76		SE	1	Rain.
16	8	0	44,0	44,0	29,06	0,116	SSW	1	Cloudy.
	2	0	48,0	46,5	29,19		SW	1	Fair.

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for January 1778.

	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches	Rain. Inch.	Winds. Points.		Weather.
Jan. 17	8	0	40,0	45,5	29,50	0,118	SSW	1	Fair.
	2	0	48,0	48,5	29,52		SW	1	Fine.
18	8	0	35,0	43,5	29,58		SE	1	Foggy.
	2	0	45,0	47,0	29,48		S by E	1	Fair.
19	8	0	44,0	44,0	29,21	0,167	SW	1	Cloudy.
	2	0	46,0	46,5	29,29		SW	2	Fair.
20	8	0	43,0	45,0	29,44	0,147	SW	2	Fair.
	2	0	44,5	48,5	29,49		SW	2	Fine.
21	8	0	40,0	45,0	29,09	0,297	SW	1	Cloudy.
	2	0	46,0	47,5	29,20		SW	1	Cloudy.
22	8	0	39,0	44,0	29,01		SSW	1	Fair.
	2	0	44,0	44,5	28,95		SW	1	Fine.
23	8	0	41,0	44,0	29,45	0,060	SW	1	Foggy.
	2	0	45,0	40,0	29,49		SW	1	Cloudy.
24	8	0	39,0	44,0	29,38	0,020	NE by N	1	Cloudy.
	2	0	42,0	45,0	29,41		NE	1	Cloudy.
25	8	0	35,5	40,0	29,39	0,435	NE	2	Rain.
	2	0	35,0	40,0	29,55		NE	3	Rain and snow.
26	8	0	30,0	38,0	30,21	0,126	N by E	1	Fair and frosty.
	2	0	36,0	38,0	30,28		NE	1	Fine.
27	8	0	25,0	32,5	30,14		NE	1	Fair and frosty.
	2	0	32,0	33,0	30,01		NE	1	Fine.
28	8	0	22,0	30,0	30,03		SW	1	Frosty.
	2	0	32,5	31,5	30,13		WSW	1	Cloudy.
29	8	0	34,5	33,0	30,08		S by W	1	Fair.
	2	0	40,5	35,0	30,11		NW	1	Foggy.
30	8	0	37,0	37,0	30,16		S by E	1	Cloudy.
	2	0	43,0	39,0	30,13		SW by W	1	Fine.
31	8	0	33,0	36,5	30,13		E	1	Foggy.
	2	0	39,0	38,0	30,11		SE	1	Cloudy.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Feb. 1	8	0	35,5	37,5	30,16	0,012	NE	1	Cloudy.
	2	0	38,0	40,0	30,18		NE	1	Cloudy.
2	8	0	34,5	37,5	30,21		NE	1	Cloudy.
	2	0	36,0	37,5	30,24		NE	1	Cloudy.
3	8	0	35,0	37,5	30,24		SE	1	Cloudy.
	2	0	38,0	40,0	30,32		SE	1	Cloudy.
4	8	0	37,0	40,0	30,22		SE	1	Cloudy.
	2	0	37,5	41,0	30,13		SE	1	Cloudy.
5	8	0	34,5	38,5	29,53		SE	1	Cloudy.
	2	0	41,0	40,0	29,86		SE	1	Fair.
6	8	0	38,5	39,5	29,64		E by N	1	Foggy.
	2	0	47,0	41,5	29,52		E by N	1	Fine.
7	8	0	44,0	43,0	29,52		SE	1	Cloudy.
	2	0	48,0	44,5	29,67		SE	1	Fair.
8	8	0	40,0	43,0	29,86	0,047	SW	1	Fair.
	2	0	48,0	45,0	29,75		SSW	2	Fine.
9	8	0	40,0	44,5	29,84		E	1	Cloudy.
	2	0	48,0	46,5	29,95		E	1	Fine.
10	8	0	34,5	41,5	30,09		E by N	1	Foggy.
	2	0	37,0	44,5	30,07		E by N	1	Cloudy.
11	8	0	35,0	40,0	30,25		NE by E	1	Cloudy.
	2	0	37,0	41,5	30,22		NE	1	Fair.
12	8	0	34,0	38,5	30,35		NE	1	Cloudy.
	2	0	36,0	39,0	30,35		NE	1	Fair.
13	8	0	33,0	38,0	30,35		SW	1	Fair.
	2	0	35,0	41,0	30,33		SW	1	Fine.
14	8	0	38,0	40,0	29,80	0,050	WSW	1	Fair.
	2	0	43,5	43,0	29,56		WSW	1	Rain.
15	8	0	33,5	38,0	29,46	0,038	NW	1	Fine.
	2	0	32,0	40,0	29,37		NW	1	Fine.
16	8	0	33,0	38,0	29,23	0,016	N by W	1	Cloudy.
	2	0	33,5	38,5	29,16		NNW	1	Snow and rain.

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	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8 0	33,0	36.5	29,39	0,045	N	1	Fair.
	2 0	35,0	38,5	29,39		N	1	Snow.
18	8 0	31,5	35,5	29,51	0,050	N by W	1	Snow.
	2 0	35,5	38,0	29,51		NW	1	Fine.
19	8 0	27,5	34,5	29,67		N by W	1	Fair.
	2 0	36,0	36,0	29,69		NW	1	Fine.
20	8 0	33,5	36,5	29,69		NW	1	Cloudy.
	2 0	40,0	38,5	29,74		NW	1	Cloudy.
21	8 0	41,0	40,0	29,99	0,070	W	1	Foggy.
	2 0	49,0	43,0	30,05		W	1	Rain.
22	8 0	46,0	45,0	29,99	0,010	WSW	1	Fair.
	2 0	52,0	47,5	29,96		WSW	1	Fine.
23	8 0	49,0	48,0	29,59		SW	2	Cloudy.
	2 0	48,0	48,0	29,59		SW	2	Fair.
24	8 0	35,0	40,0	29,27	0,072	WSW	2	Fair.
	2 0	46,0	43,5	29,29		SW	2	Fine.
25	8 0	36,0	39,5	29,23		NNW	2	Fair.
	2 0	35,0	39,0	29,38		NNW	2	Rain.
26	8 0	39,5	34,0	29,62	0,020	NNW	2	Fine.
	2 0	35,5	35,5	29,68		N by W	2	Fine.
27	8 0	25,5	32,5	29,78		S by W	1	Fair and frosty.
	2 0	35,0	38,0	29,59		SW	1	Fair.
28	8 0	37,0	35,0	29,15	0,097	W by S	1	Fair.
	2 0	44,0	37,0	29,19		NW	1	Fine.

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	Time. H. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 1	8 0	31,5	36,0	29,17	0	ENE	1	Fair and frosty.
	2 0	44 0	38,0	29,17		SE	1	Fine.
2	8 0	35,5	38,0	29,21	0,326	NNE	1	Rain.
	2 0	41,0	40,0	29,18		NE	1	Fair.
3	7 0	34,5	38,0	29,27		NE by N	1	Fair.
	2 0	42,0	40,0	29,34		NE	1	Fine.
4	7 0	36,0	38,0	29,42		NNW	1	Fair.
	2 0	40,0	40,0	29,46		NW	1	Fair.
5	7 0	34,0	37,0	29,37		NE by N	1	Fair.
	2 0	40,0	39,0	29,37		NE	1	Rain.
6	7 0	34,5	38,0	29,43		WNW	1	Fair.
	2 0	41,0	40,0	29,54		NW	1	Fair.
7	7 0	36,0	39,0	29,75	0,037	NE by N	1	Rain.
	2 0	40,0	40,0	29,76		NE	1	Fair.
8	7 0	35,5	38,0	29,69	0,055	NE	1	Rain.
	2 0	40,0	39,5	29,70		ENE	1	Cloudy.
9	7 0	36,0	38,5	29,73		NE	1	Cloudy.
	2 0	39,0	41,0	29,75		NE	1	Fair.
10	7 0	37,0	38,5	29,72		NW	1	Fair.
	2 0	42,0	42,5	29,78		NW	1	Fair.
11	7 0	39,0	41,0	29,94		ENE	1	Cloudy.
	2 0	46,0	45,0	30,06		NE	1	Fair.
12	7 0	38,5	42,0	30,21		ENE	1	Fair.
	2 0	41,0	42,5	30,27		NE	1	Fair.
13	7 0	32,5	39,0	30,36		NE by E	1	Fine.
	2 0	45,0	41,0	30,43		NE	1	Fine.
14	7 0	26,0	35,0	30,38		N	1	Fair.
	2 0	41,0	39,0	30,31		NNE	1	Fine.
15	7 0	36,0	38,0	30,26		ENE	1	Fair.
	2 0	42,5	39,5	30,22		ENE	1	Fair.
16	7 0	37,5	39,0	30,19		NE	1	Fair.
	2 0	40,0	41,5	30,19		NE	1	Fair.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 17	7	0	36,0	40,0	30,16		SE	1	Fair.
	2	0	49,0	44,5	30,16		SE	1	Fine.
18	7	0	38,0	42,0	30,03		SW	1	Fine.
	2	0	57,0	47,0	30,01		SW	1	Fine.
19	7	0	41,5	46,0	29,97		SSW	1	Fair.
	2	0	48,0	48,5	29,88		NW	1	Fine.
20	7	0	38,0	45,0	29,59		E by N	1	Fair.
	2	0	45,0	47,0	29,53		E by N	1	Fair.
21	7	0	32,0	47,0	29,50		SW by S	1	Fair.
	2	0	57,0	49,0	29,50		SW	1	Fine.
22	7	0	49,0	50,0	29,25	0,152	SW	2	Fine.
	2	0	55,5	52,5	29,22		SW	2	Fine.
23	7	0	48,0	51,0	29,13	0,020	SW	1	Rain.
	2	0	51,5	52,5	29,02		WSW	2	Cloudy.
24	7	0	42,0	49 0	29,25	0,271	SW	1	Fine.
	2	0	47,5	50,0	29,17		SSW	2	Rain.
25	7	0	37,5	46,5	29,21	0,220	NNE	2	Cloudy.
	2	0	44,0	46,5	29,46		NNE	2	Fine.
26	7	0	34,0	40,5	30,00		NNW	1	Fine.
	2	0	46,5	43,0	30,12		SW	1	Fine.
27	7	0	36,0	41,0	30,22		SW	1	Fine.
	2	0	49,0	44,0	30,24		SW	1	Fine.
28	7	0	41,0	43,0	30,12		SSE	2	Rain.
	2	0	45,0	44,0	30,09		SW	2	Rain.
29	7	0	49,0	46,0	30,04	0,150	S by E	1	Fair.
	2	0	60,0	50,0	30,06		S by E	1	Fine.
30	7	0	51,0	50,0	29,94		SW	1	Fair.
	2	0	57,0	53,0	29,91		SW	1	Fair.
31	7	0	53,0	54,0	29,89	0,012	WSW	1	Fair.
	2	0	60,0	58,0	29,93		SW	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches	Rain. Inch.	Winds. Points.	Str.	Weather.
April 1	7	0	50,5	53,5	29,96	0,055	SW	1	Fair.
	2	0	55,0	55,5	29,93		SW	1	Fine.
2	7	0	48,0	51,0	29,61		SE	1	Fair.
	2	0	65,0	55,0	29,59		SE	1	Fine.
3	7	0	48,5	54,0	29,40		SW	1	Fair.
	2	0	57,0	60,0	29,56		SW	1	Fine.
4	7	0	49,5	54,5	29,52	0,048	SW b W	1	Fair.
	2	0	58,5	57,5	29,57		S by W	2	Fine.
5	7	0	45,0	52,0	29,82	0,103	SSW	2	Fine.
	2	0	57,5	54,0	29,84		SSW	2	Fine.
6	7	0	47,5	52,0	29,82		SSW	2	Fine.
	2	0	58,5	55,0	29,82		SW	2	Fine.
7	7	0	49,0	53,0	30,03	0,032	NW	1	Rain.
	2	0	59,0	56 0	30,16		NW	1	Fair.
8	7	0	46,0	52,0	30,20		NE	1	Fair.
	2	0	61,0	56,0	30,13		ESE	1	Fine.
9	7	0	50,5	53,0	30,03		NE by E	1	Fine.
	2	0	66,0	61 0	30,02		ESE	1	Fine.
10	7	0	49,5	55,0	30,14		NE by N	1	Fine.
	2	0	69,5	60,0	30,20		NE	1	Fine.
11	7	0	48,0	57,0	30,23		NE	1	Fine.
	2	0	69,0	60,0	30,24		NE	1	Fine.
12	7	0	50,0	57,0	30,23		SSW	1	Fine.
	2	0	71,5	63,0	30,17		NNW	1	Fine.
13	7	0	51,0	58,0	30,01		NNW	1	Fair.
	2	0	67,5	62,0	29,87		NW	1	Fine.
14	7	0	42,5	53,0	29,82	0,029	NW b W	2	Fine.
	2	0	49,0	52,0	29,79		NW	2	Fine.
15	7	0	39,0	45,5	29,67	0,016	NW b N	2	Fair.
	2	0	47,0	45,0	29,62		NW	2	Fine.
16	7	0	34,5	43,0	29,59		NW b W	1	Fine.
	2	0	48,0	47,5	29,65		NW	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Apr. 17	7	0	41,0	45,0	29,64	0,026	SW	1	Fair.
	2	0	51,0	49,5	29,61		SW	1	Fine.
18	7	0	40,0	46,0	29,81	0,023	SW by S	1	Fine.
	2	0	54,0	51,0	29,95		SW	1	Fine.
19	7	0	47,0	47,5	29,94		SW by W	1	Fair.
	2	0	59,0	52,0	29,89		SW	1	Fine.
20	7	0	44,0	50,0	29,71	0,119	W by N	1	Fine.
	2	0	56,0	54,0	29,66		NNW	1	Fine.
21	7	0	41,0	47,0	29,44	0,030	NW	1	Fine.
	2	0	45,0	50,0	29,48		NW	1	Fair.
22	7	0	37,0	44,0	29,56	0,040	N by W	2	Fair.
	2	0	40,0	46,0	29,65		NW	2	Rain.
23	7	0	36,5	42,5	29,72	0,023	N by W	1	Fine.
	2	0	47,0	46,0	29,73		NW	1	Fine.
24	7	0	30,0	42,0	29,74		N by E	1	Fine.
	2	0	47,0	46,5	29,78		NE	1	Fine.
25	7	0	42,0	44,0	29,89		NE	1	Fine.
	2	0	51,5	48,5	29,91		NE	1	Fine.
26	7	0	39,5	43,5	29,73		ENE	1	Fine.
	2	0	52,5	47,0	29,65		NE	1	Fine.
27	7	0	41,0	45,0	29,45	0,025	NE	1	Rain.
	2	0	52,0	50,0	29,51		NE	1	Fair.
28	7	0	44,5	47,0	29,50	0,045	ENE	1	Fair.
	2	0	51,0	50,5	29,51		NE	1	Cloudy.
29	7	0	44,0	48,0	29,48	0,010	ENE	1	Fair.
	2	0	53,5	52,0	29,39		NE	1	Fine.
30	7	0	46,0	49,5	29,30	0,110	NE	1	Fair.
	2	0	58,0	53,0	29,32		NE	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 1	7	0	49,0	52,0	29,34		ENE	1	Cloudy.
	2	0	52,5	33,5	29,43		E by S	1	Rain.
2	7	0	50,0	53,0	29,61	0,327	S by E	1	Fine.
	2	0	58,5	55,0	29,62		S by E	1	Rain.
3	7	0	51,0	54,0	29,55	0,155	E by S	1	Rain.
	2	0	61,0	57,5	29,54		SE	1	Rain.
4	7	0	53,0	56,0	29,55	0,124	SSW	1	Rain.
	2	0	65,0	62,5	29,61		SW	1	Fine.
5	7	0	54,0	57,0	29,74		SSW	1	Fair.
	2	0	61,5	59,0	29,94		SW	1	Fine.
6	7	0	50,0	55,5	29 98	0,038	S by E	1	Fair.
	2	0	59,0	57,5	29,95		SE	2	Fair.
7	7	0	54,0	56,5	29,82	0,020	S by W	2	Cloudy.
	2	0	61,5	58,5	29,85		SW	1	Fine.
8	7	0	52,5	56,0	29,94	0,012	S by E	1	Fine.
	2	0	61,0	59,0	29,94		SE	1	Cloudy.
9	7	0	53,0	54,0	29,91		S by W	1	Fair.
	2	0	58,5	58,0	29,96		SW b W	2	Cloudy.
10	7	0	50,0	55,0	30,20	0,046	SW	1	Fine.
	2	0	66,0	59,0	30,20		SW	1	Fine.
11	7	0	54,5	56,0	30,10		SW by S	1	Fine.
	2	0	67,0	64,5	30,03		SW	1	Fine.
12	7	0	56,5	58,0	29,70		SW	1	Fine.
	2	0	65,5	63,0	29,66		SW	2	Fine.
13	7	0	53,0	59,0	29,55	0,034	S by E	1	Rain.
	2	0	64,5	62,0	29,59		SW	1	Fine.
14	7	0	55,0	59,5	29,65	0,010	S by E	2	Fine.
	2	0	67,0	61,5	29,79		S by E	2	Fine.
15	7	0	52,5	59,0	30,03	0,010	S by W	1	Fine.
	2	0	63,5	61,0	30,04		SW	1	Fine.
16	7	0	55,5	59,0	29,97		S by W	1	Cloudy.
	2	0	68,0	61,5	29,99		SW	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 17	7	0	54,0	58,5	29,73	0,075	SW	2	Cloudy.
	2	0	59,0	61,5	29,67		SW	3	Cloudy.
18	7	0	51,0	58,0	29,68	0,114	SW	2	Fine.
	2	0	61,5	59,5	29,75		SW	2	Fine.
19	7	0	52,0	56,0	30,00		WSW	1	Fine.
	2	0	63,0	60,0	30,14		SW	1	Fine.
20	7	0	53,0	57,0	30,27		WSW	1	Fair.
	2	0	62,5	59,0	30,28		WSW	1	Fair.
21	7	0	55,0	58,0	30,24		NNE	1	Cloudy.
	2	0	66,0	61,0	30,20		NE	1	Fine.
22	7	0	54,5	60,0	29,96		W by S	1	Fair.
	2	0	58,5	59,5	29,92		NW b N	1	Rain.
23	7	0	50,5	57,0	30,07	0,074	NNW	1	Fair.
	2	0	60,0	58,0	30,12		NW	1	Fine.
24	7	0	51,5	56,0	30,08		SSW	1	Fine.
	2	0	62,0	57,0	30,05		SW	1	Fine.
25	7	0	51,0	54,5	29,41		NE	1	Fair.
	2	0	58,0	56,0	29,76		NE	1	Fine.
26	7	0	53,0	56,5	29,91		N by W	1	Fine.
	2	0	66,0	61,5	29,98		NE	1	Fine.
27	7	0	56,0	60,0	29,88	0,042	W by N	1	Cloudy.
	2	0	63,0	62,0	29,88		WNW	1	Fair.
28	7	0	54,0	59,0	30,03	0,052	NW	1	Fine.
	2	0	65,0	61,0	30,11		NW	1	Fine.
29	7	0	57,0	60,0	30,08		NW	1	Cloudy.
	2	0	68,5	64,0	30,03		NW	1	Fine.
30	7	0	56,0	61,5	30,07		NE	1	Fine.
	2	0	71,5	65,0	30,02		SW	1	Fair.
31	7	0	57,0	62,0	30,00		SSW	1	Fair.
	2	0	66,0	64,0	30,04		NE	1	Cloudy.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches	Rain. Inch.	Winds. Points.	Str.	Weather.
June 1	7	0	57,5	63,0	30,03		SSW	1	Fine.
	2	0	72,0	67,0	30,03		SW	1	Fine.
2	7	0	57,0	61,0	30,06		SW	2	Fine.
	2	0	66,5	63,0	30,02		SW	2	Fine.
3	7	0	52,0	60,0	29,84	0,012	SW	1	Fine.
	2	0	62,0	61,5	29,84		SSW	1	Fine.
4	7	0	53,5	60,0	29,75		SSW	1	Fine.
	2	0	59,0	61,0	29,75		SW	1	Fine.
5	7	0	51,0	58,5	29,82	0,505	SW	1	Fine.
	2	0	61,5	61,5	29,88		SW	1	Fine.
6	7	0	59,5	60,5	29,79		SSW	1	Fine.
	2	0	73,0	63,5	29,75		SW	2	Fine.
7	7	0	58,0	63,0	29,64		WSW	1	Fair.
	2	0	69,0	65,0	29,71		SW	1	Fine.
8	7	0	56,5	62,0	29,85		SW	1	Fine.
	2	0	68,5	65,5	29,93		SW	1	Fine.
9	7	0	57,0	62,0	30,07		SW	1	Fair.
	2	0	58,0	61,5	30,10		SW	1	Rain.
10	7	0	55,5	62,0	30,17	0,220	W by S	1	Fine.
	2	0	70,5	66,0	30,17		SSW	1	Fine.
11	7	0	59,5	60,0	30,14		SSW	1	Fair.
	2	0	72,0	67,0	30,19		SW	1	Fine.
12	7	0	64,0	66,0	30,16		SW	1	Fine.
	2	0	78,5	71,5	30,19		SW	1	Fine.
13	7	0	63,0	68,5	30,16		SSW	1	Fair.
	2	0	71,5	70,0	30,24		SW	1	Fine.
14	7	0	60,0	66,0	30,36		N by E	1	Fine.
	2	0	70,0	68,5	30,36		NE	1	Fine.
15	7	0	53,5	65,0	30,25	0,140	NNW	1	Fine.
	2	0	63,0	63,5	30,19		NW	1	Fine.
16	7	0	54,0	61,0	30,15	0,016	W by N	1	Fair.
	2	0	64,0	62,0	30,18		NW	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom.	Rain.	Winds. Points.	Str.	Weather.
					Inches.	Inch.			
June 17	7	0	58,5	61,0	30,12		SW	1	Fair.
	2	0	58,0	55,0	30,12		SW	1	Fair.
18	7	0	61,0	64,0	30,03	0,064	N by W	1	Fair.
	2	0	71,0	66,5	30,02		N by W	1	Fine.
19	7	0	63,5	66,5	30,09		N by E	1	Fair.
	2	0	73,5	71,0	30,13		NE	1	Fine.
20	7	0	61,0	67,0	30,18		NNE	1	Fair.
	2	0	74,0	70,5	30,22		NE	1	Fine.
21	7	0	62,0	67,0	30,29		SE	1	Fair.
	2	0	74,0	78,0	30,25		SW	1	Fine.
22	7	0	62,0	68,0	30,22		NW	1	Cloudy.
	2	0	76,5	72,0	30,21		NW	1	Fine.
23	7	0	62,5	70,0	30,22		N by W	1	Fine.
	2	0	76,0	72,5	30,17		NE	1	Fine.
24	7	0	64,5	68,5	30,08		E by N	1	Fair.
	2	0	73,5	72,5	30,02		E by N	2	Fine.
25	7	0	68,0	68,5	29,99		ENE	1	Fair.
	2	0	68,0	70,0	30,07		SW	1	Cloudy.
26	7	0	63,0	69,0	30,10	0,030	NW	1	Fine.
	2	0	78,5	72,0	30,16		NW	1	Fine.
27	7	0	66,5	70,0	30,06		SSE	1	Fine.
	2	0	82,0	75,5	30,14		SE	1	Fine.
28	7	0	63,5	70,0	29,86		SW	1	Fine.
	2	0	75,0	73,0	29,87		SW	1	Fine.
29	7	0	63,0	70,0	29,88		NNE	1	Fine.
	2	0	76,5	73,5	29,87		SW	1	Fine.
30	7	0	60,0	68,0	29,90	0,029	NE	1	Cloudy.
	2	0	77,5	73,0	29,93		NE	1	Fine.

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	Time. H.	M.	Therm without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
July 1	7	0	65,0	70,0	29,85		S by E	1	Cloudy.
	2	0	68,0	70,0	29,81		SE	1	Cloudy.
2	7	0	63,5	67,5	30,08		W	1	Fair.
	2	0	78,0	72,5	30,12		W	1	Fine.
3	7	0	63,0	69,0	30,01		S by W	1	Fine.
	2	0	75,0	72,5	29,98		SW	1	Fine.
4	7	0	65,0	70,0	30,02		S by W	1	Fair.
	2	0	78,0	73,0	30,08		SW	1	Fine.
5	7	0	70,5	72,0	29,96		ESE	1	Fine.
	2	0	82,0	78,5	29,95		SW	1	Fine.
6	7	0	67,5	76,0	30,00	0,204	W by S	1	Fine.
	2	0	78,0	76,0	30,05		SW	1	Fine.
7	7	0	61,5	72,5	30,04		NWbW	1	Fine.
	2	0	74,0	72,0	30,02		NW	1	Fair.
8	7	0	62,5	70,5	30,15		NW	1	Fine.
	2	0	73,5	72,5	30,24		NW	1	Fine.
9	7	0	63,0	66,0	30,28		NW	1	Fine.
	2	0	80,0	73,5	30,29		NW	1	Fine.
10	7	0	66,5	69,5	30,20		W	1	Fair.
	2	0	82,5	79,0	30,20		W by N	1	Fine.
11	7	0	66,0	75,0	30,19		NW	1	Fine.
	2	0	81,0	77,0	30,19		NW	1	Fine.
12	7	0	68,5	75,0	30,23		NW	1	Fair.
	2	0	79,0	76,0	30,26		NW	1	Fine.
13	7	0	72,5	77,0	30,26		ENE	1	Fine.
	2	0	86,0	81,0	30,19		SSW	1	Fine.
14	7	0	71,5	78,0	30,11		W by S	1	Fine.
	2	0	86,0	81,0	30,08		SW	1	Fine.
15	7	0	63,0	71,5	30,19		NE	1	Fair.
	2	0	77,5	76,5	30,23		NE	1	Fine.
16	7	0	67,0	72,0	30,11		SE	1	Fine.
	2	0	83,5	79,5	30,06		SW	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
July 17	7	0	66,0	73.0	30,04		NNE	1	Fair.
	2	0	72,0	73,0	30,14		NE	1	Fair.
18	7	0	62,0	68,5	30,17		SE	1	Fine.
	2	0	78,0	72,0	30,12		ESE	1	Fair.
19	7	0	64,0	65,5	29,97		E by S	1	Fine and clear.
	2	0	78,0	72,0	29,94		SE	1	Clear.
20	7	0	66,5	71,0	29,57		NE	1	Clear, th. and l. laſt night.
	2	0	69,0	73,0	29,52		S by W	1	Much th. l. and heavy rain.
21	7	0	62,5	66,0	29,48	0,990	N by E	1	Fair.
	2	0	72,0	75,0	29,54		NE	1	Clear.
22	7	0	58,5	67,5	29,76	0,186	W by S	1	Cloudy.
	2	0	67,5	66,5	29,85		W by S	1	Cloudy.
23	7	0	61,0	65,5	29,67	0,165	SSW	2	Rain, much wind laſt n.
	2	0	62,0	66,5	29,72		SSW	1	Rainy.
24	7	0	57,0	64,5	29,72	0,175	SSW	1	Rain laſt night.
	2	0	59,0	66,0	29,69		SW	1	Much rain.
25	7	0	58,5	64,5	29,62	0,872	SE	2	Rainy.
	2	0	68,5	68,0	29,57		SE	2	Rainy.
26	7	0	60,0	64,5	29,78	0,209	SW	2	Fine, much wind laſt n.
	2	0	68,5	67,0	29,84		SW	2	Fine, after much w. & ſhow.
27	7	0	61,5	65,5	29,82	0,015	S by W	1	Cloudy.
	2	0	74,0	68,5	29,82		SW	1	Fine.
28	7	0	64,0	67,0	29,72	0,082	SW	2	Cloudy, wind and r. laſt n.
	2	0	68,5	68,0	29,75		SW	2	Showery.
29	7	0	60,0	66,0	29,79	0,046	WSW	1	Fair.
	2	0	70,0	68,5	29,99		SW	1	Fine.
30	7	0	60,5	63,0	29,88		S by E	1	Cloudy.
	2	0	72,5	78,0	29,80		SE	1	Cloudy.
31	7	0	59,0	65,0	29,77	0,209	S by E	1	Cloudy.
	2	0	65,0	65,0	29,83		SW	1	Much rain, th. and lightn.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points	Str.	Weather.
Aug. 1	7	0	58,5	63,0	30,10	0,930	NNE	1	Cloudy.
	2	0	72,0	66,0	30,19		NE	1	Fine.
2	7	0	63,0	65,5	30,20		SW	1	Fine.
	2	0	74,5	68,5	30,22		SW	2	Fine.
3	7	0	63,0	68,0	30,23		SSW	1	Fine.
	2	0	76,0	72,0	30,25		SW	1	Fine.
4	7	0	67,0	70,0	30,14		S by W	1	Fair.
	2	0	80,0	73,5	30,07		SW	1	Fine.
5	7	0	63,5	68,5	29,93		SW	1	Fine.
	2	0	78,0	73,5	29,89		SW	1	Fine.
6	7	0	67,5	72,0	29,84		SW by W	1	Fair.
	2	0	75,0	73,5	29,87		SW	1	Fine.
7	7	0	67,5	69,5	29,88		SW	1	Cloudy.
	2	0	78,0	73,5	29,94		NW	1	Fine.
8	7	0	59,5	68,0	30,01		NNE	1	Cloudy.
	2	0	73,5	70,5	30,07		NE	1	Fine.
9	7	0	61,0	67,0	30,09		SW	1	Fine.
	2	0	74,5	70,5	30,09		SW	2	Cloudy.
10	7	0	62,5	69,0	30,11		W by S	1	Cloudy.
	2	0	76,0	72,5	30,14		NW	1	Fine.
11	7	0	64,5	70,5	30,03		SW	1	Cloudy.
	2	0	73,0	71,5	30,05		SW	2	Cloudy.
12	7	0	62,0	69,0	30,04		SW	1	Fine.
	2	0	75,0	72,5	30,08		SW	1	Fine.
13	7	0	63,5	69,0	29,91		SW	1	Cloudy.
	2	0	75,5	72,5	29,90		S by W	1	Fine.
14	7	0	65,0	69,5	29,59		SW	2	Fine.
	2	0	68,0	70,0	29,76		SW	2	Cloudy and much wind.
15	7	0	54,5	65,5	30,04	0,065	SW	1	Fine.
	2	0	70,0	68,5	30,15		SW	1	Fine.
16	7	0	56,5	59,5	30,33		NE	1	Fine.
	2	0	72,0	65,0	30,39		SW	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 17	7	0	56,0	63,5	30,40		NE by E	1	Fine.
	2	0	72,0	69,0	30,36		NE by E	1	Fine.
18	7	0	59,5	65,0	30,24		NE	1	Fine.
	2	0	78,0	71,0	30,21		NE	1	Fine.
19	7	0	60,0	66,5	30,20		NE	1	Fine.
	2	0	79,5	72,5	30,19		SSE	1	Fine.
20	7	0	57,0	68,0	30,19		SE	1	Fair.
	2	0	78,5	71,5	30,16		SW	1	Fine.
21	7	0	62,0	69,0	30,20		NE	1	Fine.
	2	0	78,5	72,0	30,17		SE	1	Fine.
22	7	0	60,0	68,5	30,10		NE by N	1	Cloudy.
	2	0	76,5	73,0	30,09		SSE	1	Fine.
23	7	0	62,0	69,0	30,19		W by S	1	Fine.
	2	0	78,0	73,5	30,28		W by N	1	Fine.
24	7	0	60,5	68,0	30,44		WNW	1	Fine.
	2	0	78,0	73,5	30,51		NW	1	Fine.
25	7	0	65,5	71,5	30,49		NW bW	1	Fine.
	2	0	82,0	76,5	30,45		SW	1	Fine.
26	7	0	60,5	65,0	30,35		NNE	1	Fair.
	2	0	70,0	70,5	30,37		NE	1	Cloudy.
27	7	0	56,0	65,0	30,36		NE by N	1	Fair.
	2	0	65,0	65,5	30,33		N by E	2	Cloudy.
28	7	0	55,0	61,5	30,40		NE	2	Fine.
	2	0	69,0	65,0	30,43		NE	2	Fine.
29	7	0	52,0	60,5	30,44		N by W	1	Fine.
	2	0	66,0	63,5	30,38		NW	1	Fine.
30	7	0	55,0	61,0	30,08		WSW	1	Fine.
	2	0	68,0	64,5	29,98		W by N	1	Cloudy.
31	7	0	47,0	57,5	30,03	0,044	NW	1	Fine.
	2	0	61,0	59,5	30,14		NW	2	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Sept. 1	7	0	44,5	55,0	30,13		SW	1	Fair.
	2	0	64,0	59,0	30,09		SW	1	Fine.
2	7	0	55,5	58,5	29,90		WNW	1	Rain.
	2	0	63,0	61,5	29,94		NW	1	Cloudy.
3	7	0	55,5	60,0	29,88	0,027	N by W	1	Cloudy.
	2	0	67,0	63,5	29,86		NW	1	Cloudy.
4	7	0	49,0	60,0	30,01		NE	1	Fine.
	2	0	65,0	62,0	29,98		NW	1	Fine.
5	7	0	50,0	59,0	30,00	0,015	NE by E	1	Fine.
	2	0	63,5	61,5	30,01		NW	1	Fine.
6	7	0	47,0	53,5	30,01		NE	1	Fine.
	2	0	64,5	59,5	29,99		NE	1	Cloudy.
7	7	0	56,0	58,0	29,87		NE	1	Cloudy.
	2	0	61,0	61,5	29,85		NE	1	Rainy.
8	7	0	53,5	59,5	29,95	0,024	NNE	1	Fine.
	2	0	68,0	63,5	30,03		NE	1	Fine.
9	7	0	52,0	59,0	30,09		NNE	1	Cloudy.
	2	0	64,5	62,0	30,05		ENE	1	Fine.
10	7	0	52,5	59,0	29,98		NE	1	Cloudy.
	2	0	65,0	62,5	29,93		SE	1	Fine.
11	7	0	55,5	59,5	29,54		E	1	Cloudy.
	2	0	54,0	60,5	29,48		NE	1	Rainy.
12	7	0	53,0	58,0	29,48	0,375	SW	1	Fair.
	2	0	61,5	61,0	29,50		W by N	2	Fine.
13	7	0	47,0	57,0	29,68	0,015	W by N	1	Fine.
	2	0	63,5	60,5	29,78		NW	1	Fine.
14	7	0	45,5	56,0	30,05		NE	1	Fair.
	2	0	63,5	59,0	30,15		NE	1	Fine.
15	7	0	46,5	54,5	30,23		NE	1	Fine.
	2	0	66,0	59,5	30,20		SE	1	Fine.
16	7	0	48,0	56,0	29,96		S by E	1	Fine.
	2	0	66,5	62,0	29,80		SE	1	Cloudy.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Sept. 17	7	0	46,0	56,0	29,86	0,146	SSW	1	Fine.
	2	0	60,0	59,0	29,89		SW	1	Fine.
18	7	0	43,5	53,5	30,04		SW by S	1	Fine.
	2	0	63,0	57,5	30,09		WNW	1	Fine.
19	7	0	51,0	56,5	30,27		SW	1	Fine.
	2	0	66,5	60,0	30,30		WSW	1	Fine.
20	7	0	50,5	56,5	30,42		ENE	1	Fine.
	2	0	68,0	62,0	30,40		S	1	Fine.
21	7	0	49,5	57,0	30,42		NE	1	Fair.
	2	0	67,5	62,0	30,42		NE by E	1	Fine.
22	7	0	53,0	58,0	30,38		NE by N	1	Cloudy.
	2	0	69,0	67,5	30,39		NE	1	Fine.
23	7	0	58,5	62,5	30,26		E by N	1	Cloudy.
	2	0	71,0	67,5	30,21		NE	1	Fine.
24	7	0	52,0	62,5	29,99		SSW	1	Fair.
	2	0	67,5	65,0	29,99		NNW	1	Fine.
25	7	0	48,0	58,5	29,98		NNE	1	Fair.
	2	0	69,5	60,5	29,94		NE	1	Fine.
26	7	0	47,0	56,0	29,62		NE	1	Cloudy.
	2	0	58,5	57,5	29,45		SSE	1	Cloudy.
27	7	0	49,0	56,0	29,15	0,100	NNE	1	Cloudy.
	2	0	49,5	56,0	29,27		N by W	2	Rainy.
28	7	0	37,0	49,0	29,94	0,033	W by S	1	Fine.
	2	0	51,0	50,5	30,07		W by S	1	Fine.
29	7	0	36,5	47,0	30,27		S by W	1	Fair.
	2	0	53,5	50,0	30,30		SW	1	Fine.
30	7	0	45,5	49,0	30,03		SW	1	Fine.
	2	0	57,0	52,0	29,86		SSW	2	Fine.

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		Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points	Winds. Str.	Weather.
Oct.	1	7	0	48,0	52,0	29,47	0,147	SSW	1	Fine.
		2	0	56,5	55,5	29,42		SW	1	Showery.
	2	7	0	47,0	52,5	29,37	0,132	WSW	1	Fine.
		2	0	54,5	54,5	29,41		SW	1	Showery.
	3	7	0	41,0	50,5	29,34	0,040	SW	1	Fair.
		2	0	54,5	53,0	29,36		NW	1	Fine.
	4	7	0	41,0	50,0	29,50		SSW	1	Fair.
		2	0	54,0	52,5	29,58		NW	1	Cloudy and ſhowery.
	5	7	0	42,0	50,0	29,66		N by W	1	Fair.
		2	0	57,0	53,5	29,69		NW	1	Fair.
	6	7	0	44,0	50,5	29,60		NE by E	1	Cloudy.
		2	0	54,5	52,5	29,54		NE	1	Cloudy.
	7	7	0	55,0	54,0	29,30	0,543	ENE	1	Rainy.
		2	0	64,5	68,5	29,27		ENE	1	Cloudy.
	8	7	0	54,0	58,0	29,30	0,560	SSE	1	Fine.
		2	0	61,5	67,0	29,29		NE	1	Fine.
	9	7	0	48,0	56,5	29,38	0,052	NE	1	Cloudy.
		2	0	59,0	59,0	29,38		NE	1	Fine.
	10	7	0	52,0	57,0	29,40	0,207	NW bN	1	Cloudy.
		2	0	51,0	56,5	29,60		NE	1	Rainy.
	11	7	0	38,5	50,5	29,75	0,010	WNW	1	Fine.
		2	0	51,0	51,5	29,76		NW	1	Fine.
	12	7	0	34,0	46,0	29,91		W by N	1	Fair.
		2	0	51,0	48,5	29,96		W by N	1	Fine.
	13	7	0	40,0	46,0	29,89		NE	1	Fair.
		2	0	53,0	49,0	29,85		NE	1	Fair.
	14	7	0	35,5	45,5	29,72		NW	1	Fine.
		2	0	51,0	49,0	29,70		NW	2	Fine.
	15	7	0	38,5	45,0	29,64		NE by N	1	Fine.
		2	0	61,0	59,5	29,79		N	1	Cloudy.
	16	7	0	36,5	40,5	29,86		N	1	Fine.
		2	0	49,0	45,5	29,92		NE	1	Fair.

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	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Oct. 17	7	0	32,0	41,0	30,00		NNE	1	Fine.
	2	0	47,0	44,0	29,98		N by E	1	Fine.
18	7	0	31,5	40,0	29,98		S by E	1	Fair and frosty.
	2	0	50,0	43,5	30,00		SE	1	Fine.
19	7	0	33,5	40,5	29,90		NE by N	1	Fine.
	2	0	47,0	43,0	29,81		NE	1	Fine.
20	7	0	40,0	41,5	29,44		NE	1	Fair.
	2	0	49,0	45,0	29,44		NE	1	Cloudy.
21	7	0	39,5	43,5	29,52	0,025	WSW	1	Foggy.
	2	0	55,0	49 0	29,58		SW	1	Fine.
22	7	0	41,5	47,0	29,64	0,028	SW	1	Fine.
	2	0	53,0	49,0	29,75		SW	1	Fine.
23	7	0	54,5	50,0	29,31	0,320	S by E	2	Rainy.
	2	0	59,0	54,0	29,21		SW	2	Rainy.
24	7	0	54,5	54,5	29,18	0,465	NW	2	Rainy.
	2	0	61,5	57,0	29,17		NW	2	Showery.
25	7	0	52,5	56,5	29,20	0,133	SW by W	2	Cloudy.
	2	0	45,0	55,0	29,31		NE	1	Rainy.
26	7	0	33,5	47,0	29,79	0,037	NW b N	1	Fine.
	2	0	45,0	47,5	29,89		NW	1	Fine.
27	7	0	34,0	42,0	29,90		NE by N	1	Fair.
	2	0	47,0	45,0	29,89		NW	1	Fine.
28	7	0	45,0	44,5	29,74		ESE	1	Cloudy.
	2	0	54,0	51,0	29,72		ESE	1	Cloudy.
29	7	0	52,0	49,0	29,60	0,111	SSE	1	Cloudy.
	2	0	56,5	53,0	29,79		SE	1	Cloudy.
30	7	0	49,5	52,0	30,02	0,060	SW by W	1	Fair.
	2	0	55,0	53,0	30,16		SW	2	Fair.
31	7	0	52,0	52,5	29,84	0,033	NW	2	Cloudy.
	2	0	57,5	55,0	29,69		SW	3	Cloudy.

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for November 1778.

	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 1	8	0	45,5	51,0	30,00	0,015	WSW	2	Fine.
	2	0	54,5	51,5	30,11		NW	2	Fine.
2	8	0	45,0	50,0	30,24		W by S	1	Fine.
	2	0	55,0	52,0	30,27		SW	1	Fine.
3	8	0	45,0	49,0	29,93		SE	1	Cloudy.
	2	0	53,5	51,5	29,79		SSE	1	Fine.
4	8	0	52,0	51,5	29,49	0,034	NE	1	Cloudy.
	2	0	47,0	51,5	29,62		SW	1	Rainy.
5	8	0	36,0	46,0	29,54	0,310	SW	1	Cloudy.
	2	0	48,0	47,5	29,36		NE	1	Cloudy.
6	8	0	44,5	48,0	29,25	0,356	SE	1	Fair.
	2	0	51,5	49,5	29,25		SE	1	Cloudy.
7	8	0	46,0	48,5	29,36	0,562	SW b W	2	Fair.
	2	0	50,0	50,0	29,50		SW	2	Fine.
8	8	0	41,0	46,5	29,44	0,143	W by S	1	Cloudy.
	2	0	46,0	47,5	29,55		NW	1	Fine.
9	8	0	39,0	45,0	29,66		S by W	1	Fair.
	2	0	46,0	47,0	29,49		SE	1	Rainy.
10	8	0	42,5	46,0	29,40	0,333	W by S	1	Fair.
	2	0	47,0	47,0	29,51		NW	1	Fair.
11	8	0	45,5	45,0	28,95	0,140	SE	2	Rainy.
	2	0	41,0	46,0	29,07		SE	2	Cloudy.
12	8	0	32,5	42,5	29,50		WSW	1	Fair.
	2	0	43,5	43,5	29,59		SW	1	Fine.
13	8	0	35,0	41,0	29,73		WSW	1	Fair.
	2	0	45,0	43,0	29,83		NE	1	Cloudy.
14	8	0	39,5	41,5	29,78		E by N	1	Foggy.
	2	0	45,0	43,0	29,71		NE	1	Cloudy and rain.
15	8	0	51,0	47,0	29,43	0,264	SSE	1	Rainy.
	2	0	55,0	48,0	29,48		SW	1	Cloudy and rainy.
16	8	0	47,0	49,0	29,58	0,149	S by W	1	Fair.
	2	0	54,5	51,0	29,64		SW	1	Rainy.

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	Time H.	M.	Therm. without	Therm. within	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 17	8	0	49,5	50,5	29,59	0,249	SSW	1	Fair.
	2	0	52,0	51,5	29,58		SW	1	Fair.
18	8	0	44,0	49,5	29,61		WSW	1	Fair.
	2	0	47,0	48,0	29,66		SW	1	Fine.
19	8	0	45,5	47,5	29,87		N by W	1	Fair.
	2	0	47,0	47,5	29,99		NW	1	Cloudy.
20	8	0	45,5	48,0	29,97		SE by E	1	Cloudy.
	2	0	48,0	48,5	29,90		SE	1	Rainy.
21	8	0	46,0	47,5	30,00	0,234	S by W	1	Cloudy.
	2	0	50,0	49,0	30,01		SW	1	Rainy.
22	8	0	49,5	49,0	29,95		SW	1	Cloudy.
	2	0	51,0	52,0	29,90		SW	1	Cloudy.
23	8	0	55,0	53,0	29,63	0,168	SW	2	Cloudy.
	2	0	57,0	52,5	29,64		SW	2	Cloudy.
24	8	0	56,5	56,0	29,53	0,263	SW	2	Fair.
	2	0	56,0	57,5	29,45		SW	3	Rainy.
25	8	0	45,0	53 0	29,73	0,261	SW	2	Fine.
	2	0	49,0	52,0	29,96		SW	2	Fine.
26	8	0	41,0	48,0	30,10		WSW	1	Fine.
	2	0	48,5	50,0	29,84		SW	1	Fine.
27	8	0	50,0	51,0	29,26	0,318	SW	3	Rain and windy.
	2	0	45,0	49,5	29,35		SW	1	Fair.
28	8	0	41,5	48,5	29,85	0,025	SW	1	Fair.
	2	0	50,0	50,0	29,95		SW	1	Fine.
29	8	0	43,5	47,0	29,98		SE by S	1	Fair.
	2	0	50,0	38,0	29,88		NE	1	Fine.
30	8	0	42,0	47,5	29,77	0,073	NW	1	Fair.
	2	0	49,0	50,0	29,86		NW	1	Fine.

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METEOROLOGICAL JOURNAL
for December 1778.

	Time. H.	Time. M.	Therm. without	Therm. within.	Barom.	Rain.	Winds. Points.	Winds. Str.	Weather.
					Inches.	Inch.			
Dec. 1	8	0	32,0	42,0	30,12		SW	1	Fair and frosty.
	2	0	40,0	41,0	30,02		SW	1	Fine.
2	8	0	36,0	40,0	29,54		SE	1	Fair.
	2	0	40,0	41,5	29,42		SW	1	Cloudy.
3	8	0	40,5	41,5	29,19	0,125	SE	1	Fair.
	2	0	45,0	46,0	29,16		SE	1	Cloudy.
4	8	0	43,0	44,5	28,55	0,495	SSE	2	Rain and much wind last n.
	2	0	44,0	43,0	28,79		SE	2	Rainy.
5	8	0	37,0	43,0	29,00	0,038	S	1	Fair.
	2	0	42,0	43,5	28,91		NNE	1	Rainy.
6	8	0	35,0	40,5	29,19	0,130	SW	1	Fair.
	2	0	40,5	44,5	29,37		SW	1	Fine.
7	8	0	50,5	44,5	29,03	0,206	SW by S	2	Fair.
	2	0	47,0	46,0	29,32		SW	2	Fair.
8	8	0	50,5	46,5	29,40	0,130	SSW	3	Rain and much wind last n.
	2	0	55,0	58,5	29,45		SSW	3	Rainy.
9	8	0	48,5	50,5	29,79	0,187	SW	1	Fair.
	2	0	56,0	53,0	29,88		SW	1	Fair.
10	8	0	49,5	52,0	29,67	0,114	S by W	1	Cloudy.
	2	0	52,0	53,0	29,64		SW	1	Cloudy.
11	8	0	51,5	52,0	29,63	0,135	SW	2	Cloudy.
	2	0	54,5	53,5	29,58		SW	2	Cloudy.
12	8	0	53,0	54,0	29,48	0,036	SW by S	2	Cloudy.
	2	0	53,0	54,5	29,38		SW	2	Fine.
13	8	0	47,5	52,5	29,13	0,051	SW	1	Fair.
	2	0	48,0	52,5	29,21		SW	1	Rain.
14	8	0	42,0	48,5	29,44	0,350	SW by S	1	Fair.
	2	0	48,0	50,0	29,39		SE	1	Rainy and cloudy.
15	8	0	41,0	46,0	29,66	0,096	W by S	2	Fair.
	2	0	44,0	46,5	29,88		SW	1	Fine.
16	8	0	34,5	41,0	30,15		SW	1	Fair.
	2	0	42,0	43,0	30,21		SW	1	Cloudy.

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for December 1778.

	Time. H.	M.	Therm. without	Therm within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Dec. 17	8	0	43,0	43,5	30,25		WSW	1	Cloudy.
	2	0	47,5	44,5	30,33		SW	1	Fine.
18	8	0	48,0	47,0	30,33		SW	1	Cloudy.
	2	0	52,0	50,0	30,36		SW	1	Cloudy.
19	8	0	49,5	51,0	30,19		SW	2	Cloudy.
	2	0	52,5	51,5	30,17		SW	1	Cloudy.
20	8	0	38,5	47,0	30,34	0,051	W by S	1	Fair.
	2	0	45,0	47,0	30,32		SW	1	Fine.
21	8	0	45,0	47,0	30,37		SW	1	Foggy.
	2	0	47,0	49,0	30,44		W	1	Foggy and cloudy.
22	8	0	44,5	47,5	30,38		WSW	1	Cloudy.
	2	0	45,0	48,5	30,35		WSW	1	Cloudy.
23	8	0	43,5	48,0	30,31	0,010	WSW	1	Rainy.
	2	0	45,0	50,0	30,36		NW	1	Cloudy and rain.
24	8	0	44,5	47,5	30,54	0,032	N by E	1	Cloudy and rainy.
	2	0	48,0	48,0	30,64		NE	1	Cloudy and rainy.
25	8	0	44,5	48,5	30,71	0,033	ESE	1	Foggy.
	2	0	46,0	49,0	30,75		ESE	1	Foggy and Cloudy.
26	8	0	34,5	44,5	30,77		SSW	1	Clear.
	2	0	40,0	45,0	30,81		SSW	1	Clear and fine.
27	8	0	33,0	40,0	30,73		SSW	1	Foggy.
	2	0	38,5	41,0	30,64		SSE	1	Cloudy.
28	8	0	39,0	41,5	30,16		SW	1	Fair.
	2	0	45,0	44,5	30,09		SW	1	Cloudy.
29	8	0	44,0	43,0	29,76	0,060	WSW	2	Rain and much wind last n.
	2	0	50,0	47,5	29,58		NW	2	Rain and cloudy.
30	8	0	35,5	42,0	29,89	0,040	W	1	Cloudy.
	2	0	42,0	43,0	29,87		SW	1	Fine.
31	8	0	48,5	45,5	29,60	0,125	W by S	2	Cloudy.
	2	0	50,0	46,5	29,18		SW	3	Cloudy.

1778.	Thermometer without.			Thermometer within.			Barometer.			Rain
	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Inches
January	48,0	22,0	37,1	48,5	30,0	38,9	30,22	28,59	29,88	2,11
February	49,0	25,5	38,2	48,0	32,5	39,6	30,35	29,15	29,82	0,52
March	60,0	31,5	42,3	58,0	35,0	43,4	30,43	29,02	29,78	1,24
April	71,5	34,5	50,0	63,0	42,0	51,3	30,24	29,30	29,76	0,73
May	71,5	49,0	58,5	65,0	52,0	58,4	30,28	29,34	29,89	1,13
June	82,0	51,0	65,0	78,0	55 0	66,3	30,36	29,64	30,05	0,95
July	86,0	58,5	70,3	81,0	63 0	71,0	30,29	29,48	29,98	3,05
Auguſt	82,0	47,0	67,0	76,5	57,5	68,0	30,51	29,59	30,31	1,03
September	71,0	36,5	56,1	67,5	47,0	58,4	30,42	29,15	29,77	0,73
October	64,5	31,5	48,3	68,5	40,0	50,3	30,16	29,17	29,63	2,90
November	57,0	32,5	47,0	57,5	38,0	45,1	30,27	28,95	29,68	3,89
December	56,0	32,0	44,7	58,5	40,0	46,7	30,81	28,55	29,85	2,44
Whole Year			52,0			53,1			29,86	20,77

VARIATION NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Daily Means.
	° ′	° ′	° ′	° ′	° ′
June 29	— —	— —	— —	22 22	22 23
30	22 7	22 22	22 25	22 18	22 18
July 1	22 7	22 28	22 27	22 13	22 19
2	22 3	22 24	22 20	22 7	22 13
3	22 7	22 27	22 30	22 18	22 20
4	22 10	22 24	22 23	22 14	22 18
5	22 8	22 25	22 27	22 24	22 21
6	22 12	22 26	22 28	22 21	22 22
7	22 9	22 29	22 43	22 14	22 24
8	22 18	22 19	22 25	22 20	22 21
9	22 10	22 26	22 28	22 28	22 23
10	22 9	22 27	22 28	22 27	22 23
11	22 9	22 26	22 24	22 21	22 20
12	22 8	22 28	22 36	22 2	22 18
13	22 10	22 30	22 30	— —	22 17
Means	22 9	22 26	22 28	22 18	

Mean of all 22° 20′ 1/4.

It was found by the method deſcribed in Phil. Tranſ. vol. LXVI. p. 387. that the variation ſhewn by the in-

ſtrument is $9'\frac{1}{4}$ too great, ſo that the mean variation is really only $22^{\circ}\ 11'$; and, therefore, as the variation at the Society's Houſe ſeems to be $15'\frac{1}{2}$ greater than it ought to be, by reaſon of the magnetiſm of the building (*vide* the ſame place) the true variation ſeems to be $21^{\circ}\ 55'\frac{1}{2}$.

The reaſon why the inſtrument now ſhews the variation $9'\frac{1}{4}$ too great, whereas before the errors was inſenſibls is, that a ſmall alteration has been made in the agate cap of the needle ſince the laſt year's obſervations, as it was thought not to have been faſtened before in a ſufficiently ſteady manner.

DIPPING

DIPPING NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 P.M.	Mean.	
	° ′	° ′	° ′	° ′	° ′	
June 29	– –	– –	– –	73 35		
30	73 20	73 20	73 15	73 10	73 16	West mark down.
July 1	73 15	73 20	73 10	73 10		
2	73 15	73 15	73 10	73 20		
3	73 10	71 50	71 45	71 50		
4	71 50	71 50	71 50	71 50	71 47	East.
5	71 50	71 45	71 45	71 40		
6	71 50	71 40	71 40	71 25		
7	71 20	71 25	71 10	71 20	71 25	West mark uppermoſt.
8	71 25	71 25	71 25	71 30		
9	71 25	71 50	71 25	71 30		
10	71 15	73 15	73 10	73 10		
11	73 20	73 30	73 10	73 40	73 16	East
12	73 10	73 10	73 0	73 10		
13	73 20	73 15	73 25	– –		

THE END OF VOL. LXIX. PART I.

ERRATA.

VOL. LXVIII. PART II.

Page. Line.

789. 12. *for* ARTEDE *read* ARTEDI

VOL. LXIX. PART I.

13. 19. *for* was *read* were

15. 5. for *ericæ forme* read *ericæ-forme*

18. 7. for *erica*, formed, *read erica*-formed

21. 13. *for* produces *read* produce

32. 8. *for* it *read* its

79. 15. *for* ſuppoſita *pro* ſuppoſito

118. 7. *for* at the ſea *read* at ſea

123. 1. *for* ⊙ *read* ○

132. 3. *for* obſervation *read* obſervations

149. 8. *for* kind *meatus* read kind of *meatus*

PHILOSOPHICAL TRANSACTIONS.

PART II.

TRANSACTIONS,

OF THE

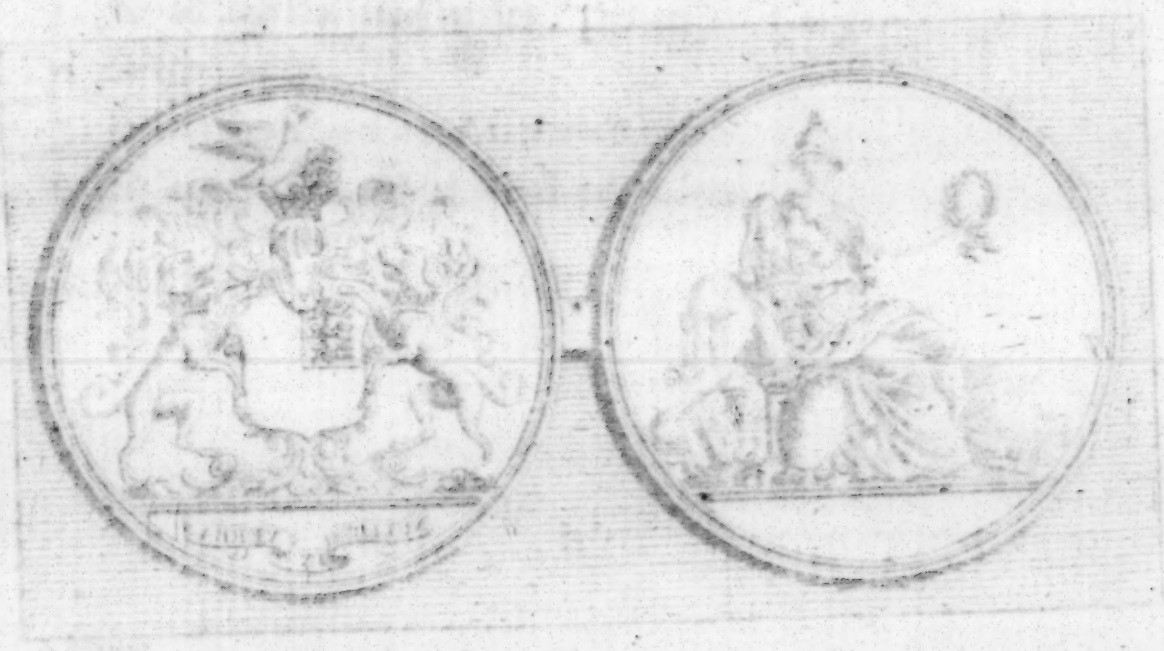
ROYAL SOCIETY

OF

PHILOSOPHICAL

TRANSACTIONS.

PART II.



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OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXIX. For the Year 1779.

PART II.

LONDON,

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MDCCLXXX.

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Read March 4, 1779.

DEAR SIR,

St. Peterſburg, October 12, 1778.

I SHALL endeavour to recollect, according to your deſire, the particulars of that part of my former letter which related to the mode of recovering people in Ruſſia, who are apparently deprived of life by the prin-

ciple emitted from burning charcoal, or by the incruſtation formed upon the inſides of the boors huts when it thaws.

People of condition in this country have double windows to their houſes in winter; but the commoner ſort have only ſingle ones, which is the reaſon that, during a ſevere froſt, there is an incruſtation formed upon the inſides of the glaſs windows. This ſeems to be compoſed of condenſed breath, perſpiration, &c. as a number of people live and ſleep in the ſame ſmall room, eſpecially in great cities. This excrementitious cruſt is farther impregnated with the phlogiſton of candles, and of the oven with which the chamber is heated.

When a thaw ſucceeds a hard froſt of long duration, and this plate of ice is converted into water, there is a principle ſet looſe, which produces all the terrible effects upon the human body which the principle emitted from charcoal is ſo well known to do in this country, where people every day ſuffer from it. However, the Ruſſians conſtantly lay the blame upon the oven, when they are affected by the thawing of the cruſt, as the effects are perfectly ſimilar, and they cannot bring themſelves to believe, that the diſſolving of ſo ſmall a portion of ice can be attended with any bad conſequence, when they daily melt larger maſſes without danger: yet the oven

does

does not at all account for the complaints brought on at this period; for, upon examination, they generally find every thing right there, and ſtill the *ugar*, or hurtful vapour, remaining in the room.

As the effects of both are ſimilar, as I have ſaid above, and likewiſe the mode of recovery, I ſhall only give you an account of the operation of the principle emitted by burning charcoal, and of the method of bringing thoſe people to life who have been *ſuffocated* by it (as I think it is erroneouſly termed); this will ſuperſede the neceſſity of giving the hiſtory of both, or rather it will be giving both at the ſame time.

Ruſſian houſes are heated by the means of ovens; and the manner of heating them is as follows. A number of billets of wood are placed in the peech or ſtove, and allowed to burn till they fall in a maſs of bright red cinders; then the vent above is ſhut up, and likewiſe the door of the peech which opens into the room, in order to concentrate the heat; this makes the tiles of which the peech is compoſed as hot as you deſire, and ſufficiently warms the apartment; but ſometimes a ſervant is ſo negligent as to ſhut up the peech or oven before the wood is ſufficiently burnt, for the red cinders ſhould be turned over from time to time to ſee that no bit of wood remains of a blackiſh colour, but that the whole maſs is

of a uniform glare (as if almoſt tranſparent) before the openings are ſhut, elſe the *ugar* or vapour is ſure to ſucceed to miſmanagement of this ſort, and its effects are as follows.

If a perſon lays himſelf down to ſleep in the room expoſed to the influence of this vapour, he falls into ſo ſound a ſleep that it is difficult to awake him, but he feels (or is ſenſible of) nothing. There is no ſpaſm excited in the *trachea arteria* or lungs to rouſe him, nor does the breathing, by all accounts, ſeem to be particularly affected: in ſhort, there is no one ſymptom of ſuffocation; but towards the end of the cataſtrophe, a ſort of groaning is heard by people in the next room, which brings them ſometimes to the relief of the ſufferer. If a perſon only ſits down in the room, without intention to ſleep, he is, after ſome time, ſeized with a drowzineſs and inclination to vomit. However, this laſt ſymptom ſeldom affects a Ruſſian, it is chiefly foreigners who are awaked to their dangers by a *nauſea*; but the natives, in common with ſtrangers, perceive a dull pain in their heads, and if they do not remove directly, which they are often too ſleepy to do, are ſoon deprived of their ſenſes and power of motion, inſomuch, that if no perſon fortunately diſcovers them within an hour after this worſt ſtage, they are irrecoverably loſt; for the Ruſſians ſay, that they

do not ſucceed in reſtoring to life thoſe who have lain more than an hour in a ſtate of inſenſibility.

The recovery is always attempted, and often effected, in this manner. They carry the patient immediately out of doors, and lay him upon the ſnow, with nothing on him but a ſhirt and linen drawers. His ſtomach and temples are then well rubbed with ſnow, and cold water, or milk is poured down his throat. This friction is continued with freſh ſnow until the livid hue, which the body had when brought out, is changed to its natural colour, and life renewed; then they cure the violent head-ach which remains by binding on the forehead a cataplaſm of black rye bread, and vinegar.

In this manner the unfortunate man is perfectly reſtored, without blowing up the lungs, as is neceſſary in the caſe of drowned perſons; on the contrary, they begin to play of themſelves ſo ſoon as the ſurcharge of phlogiſton makes its eſcape from the body.

It is well worthy of obſervation, how diametrically oppoſite the modes are of reſtoring to life, thoſe who are deprived of it by water, and thoſe who have loſt it by the fumes of charcoal: the one conſiſting in the internal and external application of heat, and the other in that of cold. It may be alledged, that the ſtimulus of the cold produces heat, and the fact ſeems to be confirmed by the Ruſſian method of reſtoring circulation in a frozen

limb

limb by means of friction with snow. But what is singular in the case of people apparently deprived of life in the manner treated of is, that the body is much warmer when brought out of the room than at the instant life is restored, and that they awake cold and shivering. The colour of the body is also changed from a livid red to its natural complexion, which, together with some other circumstances, would almost lead one to suspect, that they are restored to life by the snow and cold water some how or other freeing them from the load of phlogiston with which the system seems to be replete; for although the first application of cold water to the human body produces heat, yet, if often repeated in a very cold atmosphere, it then cools instead of continuing to heat, just as the cold bath does when a person remains too long in it.

In short, I think it is altogether a curious subject, whether you take into consideration the mode of action of the principle emitted by burning charcoal, and our phlogisticated crust; or the operation of the snow and cold water. However, I shall by no means take upon me to decide, whether the dangerous symptoms related above are produced by the air in the room being so saturated with phlogiston as to be unable to take up the proper quantity from the lungs, which occasions a surcharge in the system, according to your theory, or whether

ther so subtle a fluid may somehow find its way into the circulation, and thereby arrest the vital powers; nor shall I determine whether the livid hue of the body when brought out is changed into a paler colour by the atmosphere somehow or other absorbing and freeing the blood from the colouring principle, as you have shewn to be the case with blood out of the body: these are curious inquiries that I shall leave to your investigation. I have only endeavoured to collect facts from a number of natives who have met with this accident themselves, or have assisted in restoring others to life. It is so common a case here that it is perfectly familiar to them, and they never call in medical assistance.

I am, &c.

XXIII. *An Account of an Apparatus applied to the equatorial Inſtrument for correcting the Errors ariſing from the Refraction in Altitude. By Mr.* Peter Dollond, *Optician; communicated by the Aſtronomer Royal.*

Read March 4, 1779.

THE refraction of the atmoſphere occaſions the ſtars or planets to appear higher above the horizon than they really are; therefore, a correction for this refraction ſhould be made in a vertical direction to the horizon.

The equatorial inſtrument is ſo conſtructed, that the correction cannot be made by the arches or circles which compoſe it when the ſtar, &c. is in any other vertical arch except that of the meridian, becauſe the declination arch is never in a vertical poſition but when the teleſcope is in the plane of the meridian.

To correct this error, a method of moving the eye-tube which contains the wires of the teleſcope in a vertical direction to the horizon has been practiſed; but as the eye-tube is obliged to be turned round in order to move

move it in that direction, in the different oblique poſitions of the inſtrument, the wires are thereby put out of their proper ſituation in every other poſition of the inſtrument, except when it is in the plane of the meridian; for the equatorial wire ſhould always be parallel to the equator, that the ſtar in paſſing over the field of the teleſcope may move along with it, otherwiſe one cannot judge whether the teleſcope be ſet to the proper declination, except at the inſtant the ſtar is brought to the interſection of the wires, which is only a momentary obſervation.

The method I have now put in practice for correcting the refraction of the atmoſphere is by applying two lenſes before the object-glaſs of the teleſcope; one of them convex, and the other concave; both ground on ſpheres of the ſame radius, which in thoſe I have made is thirty feet. The convex lens is round, of the ſame diameter as the object-glaſs of the teleſcope, and fixed into a braſs frame or apparatus, which fits on to the end of the teleſcope. The concave lens is of the ſame width, but nearly two inches longer than it is wide, and is fixed in an oblong frame, which is made to ſlide on the frame the other lens is fixed into, and cloſe to it. Theſe two lenſes being wrought on ſpheres of the ſame radius, the refraction of the one will be exactly deſtroyed by that of

the other, and the focal length of the object-glaſs will not be altered by their being applied before it; and if the centers of theſe two lenſes coincide with each other, and alſo with that of the object-glaſs, the image of any object formed in the teleſcope will not be moved or ſuffer any change in its poſition. But if one of the lenſes be moved on the other, in the direction of a vertical arch, ſo as to ſeparate its center from that of the other lens, it will occaſion a refraction, and the image will change its altitude in the teleſcope. The quantity of the refraction will be always in proportion to the motion of the lens, ſo that by a ſcale of equal parts applied to the braſs frame, the lens may be ſet to occaſion a refraction equal to the refraction of the atmoſphere in any altitude. If the concave lens be moved downwards, that is, towards the horizon, its refraction will then be in a contrary direction to that of the atmoſphere, and the ſtar will appear in the teleſcope as if no refraction had taken place.

There is a ſmall circular ſpirit level fixed on one ſide the apparatus, which ſerves to ſet it in ſuch a poſition, that the centers of the two lenſes may be in the plane of a vertical arch. This level is alſo uſed for adjuſting a ſmall quadrant, which is fixed to it, and divided into degrees, to ſhew the elevation of the teleſcope when directed to the ſtar; then the quantity of refraction anſwering

ſwering to that altitude may be found by the common tables, and the concave lens ſet accordingly, by means of the ſcale at the ſide, which is divided into half minutes, and, if required, by uſing a nonius, may be divided into ſeconds.

It muſt be obſerved, that when a ſtar or planet is but a few degrees above the horizon, the refraction of the atmoſphere occaſions it to be conſiderably coloured. The refraction of the lens acting in a contrary direction would exactly correct that colour, if the diſſipation of the rays of light were the ſame in glaſs as in air; but as it is greater in glaſs than in air, the colours occaſioned by the refraction of the atmoſphere will be rather more than corrected by thoſe occaſioned by the refraction of the lens.

A drawing of the refraction apparatus is added, which may ſerve to give a more clear idea of it. See plate IV.

EXPLANATION OF THE PLATE.

AA, The circular braſs tube, which fits on to the end of the teleſcope.

BB, The oblong concave lens in its frame, which ſlides over the fixed convex lens.

C, The circular ſpirit level, which ſhews when the oblong lens is in a vertical arch.

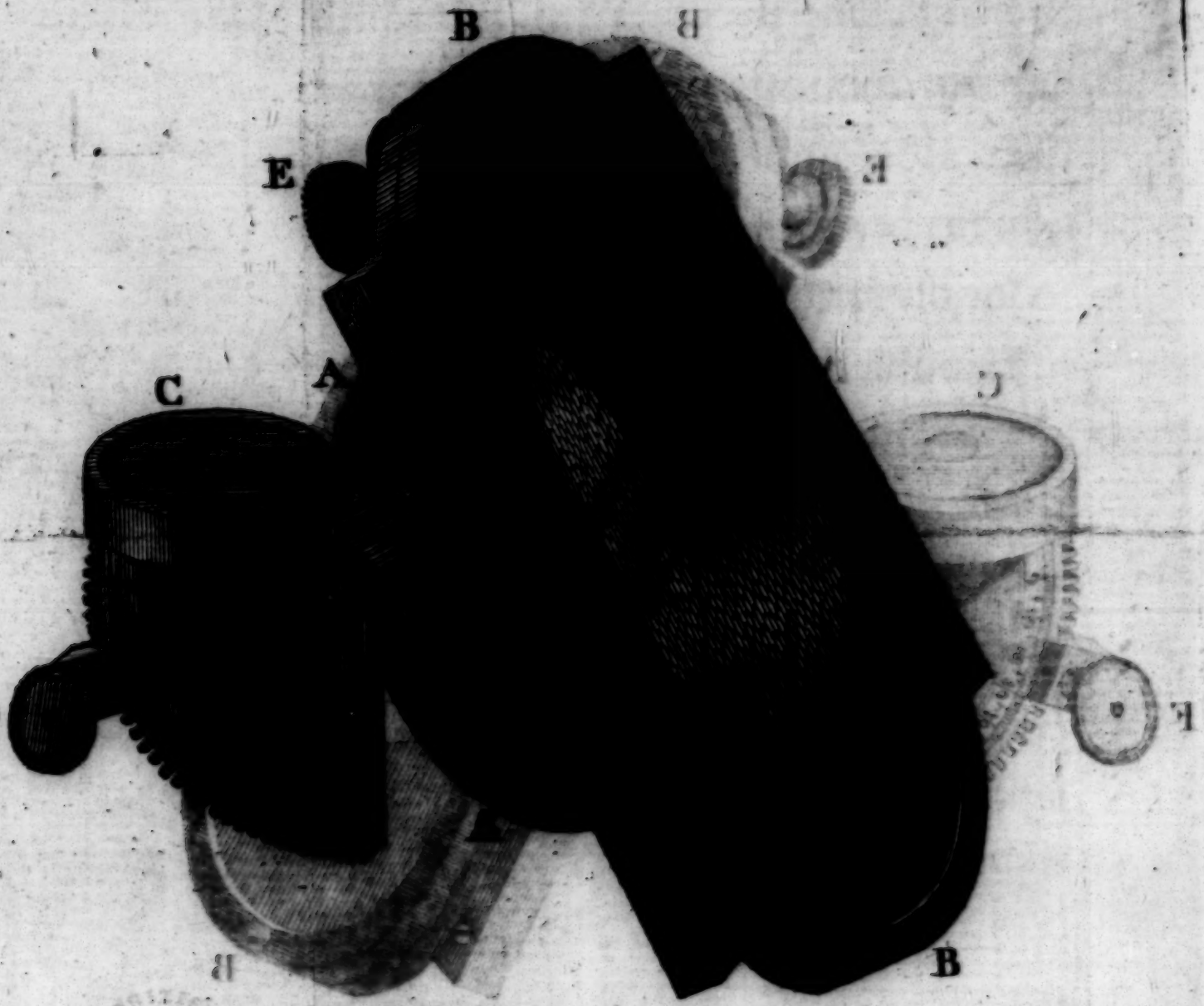
D, The quadrant to which the ſpirit level is fixed, for ſhewing the angular elevation of the teleſcope.

E, The milled head fixed to a pinion, by which the whole apparatus is turned round on the end of the teleſcope, in order to ſet the oblong lens in a vertical arch.

F, Another pinion for ſetting the quadrant to the angular elevation of the teleſcope. By means of theſe two pinions the air bubble muſt be brought to the middle of the level.

a a, Is the ſcale, with diviſions anſwering to minutes and half minutes of the refraction occaſioned by the concave lens.

Philos. Trans. Vol. LXIX. Tab. IV. p. 336.



XXIV. *Experiments and Obſervations on the inflammable Air breathed by various Animals.* *By the Abbé* Fontana, *Director of the Cabinet of Natural Hiſtory belonging to his Royal Highneſs the Grand Duke of* Tuſcany; *communicated by* John Paradiſe, *Eſq. F. R. S.*

Read March 11, 1779.

PHILOSOPHERS believed, till lately, that inflammable air had the power of killing animals who breathed it. Dr. PRIESTLEY, to whom we are much indebted for many diſcoveries and obſervations relative to inflammable air, made in conſequence of Mr. CAVENDISH'S excellent paper on that ſubject, aſſures us, that inflammable air cauſes the death of animals as readily as fixed air, and that animals die convulſed in it. The doctor adds, that water abſorbed about one quarter of the inflammable air ſhaken in it, after which a mouſe lived in it as long as it would have lived in an equal quantity of common air. This air breathed by the mouſe was ſtill inflammable, though not ſo much as before.

Mr.

Mr. SHEELE, who has made various important obſervations in chemiſtry, on the contrary aſſerts, that inflammable air not only does not kill the animals who breathe it, but that it is even good and innocent air. He relates ſome experiments to which it ſeems that nothing can be oppoſed, and they appear to contradict Dr. PRIESTLEY's obſervations. Mr. SHEELE has breathed inflammable air contained in a bladder, without receiving any hurt.

Seeing then that the experiments of theſe celebrated perſons contradicted each other, I began to ſuſpect that they might poſſibly be all true; and that their ſo contradictory effects might be owing to ſome circumſtance not yet attended to.

In order to follow ſome method in my reſearches about a point ſo delicate, and which ſo nearly intereſts human life, I firſt of all thought of aſſuring myſelf, whether or no animals could breathe inflammable air with impunity, when the receivers that contained it were immerſed in quickſilver. To this end, I introduced inflammable air, extracted both from zinc and iron, by means of the vitriolic acid, into various tubes filled with quickſilver, in which the air entered pretty free from moiſture. I then introduced various birds into thoſe tubes, and obſerved that they died in a few minutes

time, but without any apparent ſign of convulſions. Theſe experiments, having been often repeated, were conſtantly attended with the ſame event.

Being aſſured, beyond any doubt, that the inflammable air obtained from zinc or iron, and made to paſs through quickſilver, was fatal to animals; I next wiſhed to obſerve, whether it retained the ſame properties when it had paſſed through water; in which caſe the volatile ſulphurous acid, or other vapour, is abſorbed by the water; but, on trying the experiments, I found that the birds died under theſe circumſtances as under the others (though not quite ſo ſoon) ſhewing likewiſe ſome ſigns of convulſion. I introduced ſome of this ſame air that had paſſed through water into a glaſs tube full of quickſilver, by a method which makes the air loſe all its moiſture. The birds died in it in the ſame manner as when the experiment was tried upon water. In all theſe caſes the air after the animals had died in it was ſtill inflammable, nor did its exploding properties ſeem to have been at all diminiſhed.

The inflammable air extracted from zinc, and that extracted from iron, is fatal to animals even after it has been ſhaken in water for a minute's time, or ſomething longer. By ſhaking it a long time, it becomes in ſome meaſure reſpirable; but then it is decompoſed in a great meaſure,

meaſure, and becomes of another kind, although it ſtill preſerves the properties of being inflammable, but in a ſmaller degree.

Not only birds but alſo quadrupeds die in inflammable air (though not ſo ſoon) and ſhew ſome ſigns of being convulſed.

It ſeems very ſtrange, that Mr. SHEELE could breathe inflammable air with impunity, when animals obliged to breathe it were killed in a very ſhort time. Admitting his experiments to be true, there remains nothing to be ſaid, but that the inflammable air in which animals die does not occaſion death becauſe it is conveyed to the lungs, but becauſe it affects ſome other organs of the animal body expoſed to that air, and neceſſary to animal life. It is not impoſſible to occaſion death by affecting the very ſenſible nerves of the noſe; it being well known, that various liquors, as very concentrated volatile alkaly, &c. if they are inſpired through the noſe, immediately affect the ſenſes, and occaſion death if they continue to act upon the *pituitary* membrane.

In order, therefore, to try whether inflammable air killed, only becauſe it was inſpired through the noſe, I ſtopped very accurately the noſes of various birds with ſoft wax, and in this manner I introduced them into

receivers

receivers full of inflammable air extracted from zinc, and from iron, through water. The birds died within a few ſeconds, that is, juſt as they did when their noſes were unſtopped. Quadrupeds were tried after the ſame manner, and the event was the ſame.

Having in this manner exploded this new hypotheſis, there remained one more, which ſeemed to ſuggeſt a probable reaſon (ſince ſome reaſon there muſt be) for Mr. SHEELE's experiments being attended with reſults ſo different from thoſe of other experimenters. When an animal is introduced into a veſſel of inflammable air, its whole body is expoſed to that air; and it is not yet known by philoſophers what diſorders that fluid may occaſion to the animal frame. It is true that none are obſerved to be produced by other noxious kind of air; but if it be conſidered, that the vapours of ſulphur make a great impreſſion upon frogs, even when thoſe animals do not breathe them, but have their *aſpera arteria* tied up, it will not ſeem impoſſible for the inflammable air, in ſome manner or other, to act upon the body of animals. It may, perhaps, hinder the perſpiration; it may inſinuate itſelf through the pores of the ſkin; in ſhort, its action upon the body ſeems probable till experiments evince the contrary.

I therefore endeavoured to force various four-footed animals to breathe the inflammable air through the mouth only, without immerging their whole bodies into it. I chiefly uſed bladders tied to their mouths, but ſometimes I alſo made uſe of tubes which entered immediately into the wind-pipe. In both caſes the animals died in a very ſhort time: hence it became evident, not only that the inflammable air is pernicious to animal life, but that it does not act on the *body* of an animal; for I kept ſome of them immerged in inflammable air, with the mouth only out of it, and did not perceive any effect hurtful to them.

It being in this manner aſcertained, that the inflammable air could not be breathed by animals with impunity, it ſtill remained to find out the cauſe of Mr. SHEELE's miſtake. I began therefore to breathe the inflammable air contained in bladders, after the manner of Mr. SHEELE. The inflammable air uſed in my experiments was extracted from zinc and from iron by the action of the vitriolic acid, and it was received into bladders that were dry in the inſide, but a little moiſt on the outſide. The quantity of air contained in each bladder was about eighty cubic inches. The air coming out of the mattraſs paſſed through about one inch of water before it went into the bladders. At firſt I

breathed

breathed the inflammable air with a kind of fear; but finding that it occaſioned no painful impreſſion, I continued breathing it with courage as long as I could. I breathed in a bladder filled with it eleven times, beginning after a natural expiration. This air when taken out of the bladder, was ſtill inflammable, and being tried with the teſt of nitrous air it gave II–28, III+20.

Before I go farther I muſt explain the formula which I uſe to expreſs the diminution of reſpirable air, or air of other kind, when mixed with nitrous air. My method is as follows: I have a glaſs tube of about eighteen inches in length, and half an inch in diameter, cloſed at one end, and of a conſtant diameter throughout its whole length: this tube has a mark at every three inches, which marks or diviſions I call *meaſures*, and every inch is divided into twenty equal parts; ſo that every *meaſure* is divided into ſixty portions, which I call *parts*. Into this tube, by means of an inſtrument which meaſures always one conſtant quantity of air equal to one meaſure of the tube, I introduce two meaſures of reſpirable air and one meaſure of nitrous air, after which I meaſure the diminution; then I introduce a ſecond meaſure of nitrous air, and again meaſure the diminution. The whole meaſures I expreſs in Roman characters, and the

parts of a meaſure I expreſs in common numbers; for inſtance, when I ſay II−16 and II+10, the firſt expreſſion means, that after having introduced into the tube two meaſures of common air, and one meaſure of nitrous air, the ſpace occupied by the mixture of theſe two airs was two meaſures − 16 parts, or 6oths of a meaſure: and the ſecond expreſſion ſhews that, after having introduced another meaſure of nitrous air, the ſpace occupied was two meaſures + 10 parts. The reaſon and particulars of this method will be given hereafter in a paper expreſsly written upon the method of determining the degree of the ſalubrity of the air by means of nitrous and inflammable air.

Having introduced eight cubic inches of common air into the ſame bladder, I breathed it as long as I could; beginning after a natural expiration as in the experiment above related. I breathed it thirty-four times ſucceſſively, and afterwards found it very much altered, ſo that it extinguiſhed a light many times ſucceſſively. An animal introduced into a veſſel of that air immediately gave ſigns of uneaſineſs: and the air being tried with the nitrous air gave II+20, III+15; whereas, before it had been breathed, it gave with the ſame nitrous air II−15; II+18.

This

This experiment ſhews, that the air which remained in the bladder in the firſt experiment was not ſo good as that breathed thirty-four times ſucceſſively. In order to make this experiment with more preciſion, I breathed eighty cubic inches of common air, introduced into the ſame bladder, only eleven times; beginning after a natural expiration. Then I examined this air with the nitrous air, and found that it gave II-13, III+28.

Hence it is plain, that the mixture of inflammable and pulmonary air breathed eleven times is much inferior to common air breathed an equal number of times; ſo that there can remain no doubt but that inflammable air is at leaſt worſe than common air.

Willing, however, to aſcertain this matter ſtill better, I tried to breathe it immediately through a large receiver, partly immerſed in water, and ſwimming in it, ſo that the air within the receiver was of the ſame elaſticity with the external air. For this experiment I made uſe alſo of a glaſs tube bended in two different directions.

The air contained in the receiver was about 250 cubic inches. In all the trials made in this manner, I was never able to breathe the inflammable air more than three times, and even at the ſecond inſpiration I felt a great oppreſſion. As theſe experiments can be depended upon, becauſe they were often and at different times repeated,

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there ſeems to be reaſon enough to ſuſpect that the bladder might poſſibly alter the nature of inflammable air, and render it more fit for reſpiration, notwithſtanding that the mere contact of the bladder ſeemed not ſufficient to produce ſuch an effect, which is irreconcileable with other facts: yet ſome reaſon muſt certainly exiſt ſufficient to explain Mr. SHEELE's experiments, which directly prove that the inflammable air contained in bladders can be breathed with impunity.

When I breathed this air according to Mr. SHEELE's manner eleven times ſucceſſively, I not only breathed it without any inconvenience, but obſerved that the firſt inſpirations were even pleaſing; more ſo than when I breathed common air. I felt a facility of dilating the breaſt, as if the air was as light as that at the top of high mountains. I never felt a like ſenſation, even when I have breathed the pureſt dephlogiſticated air. I do not think that I was miſtaken in theſe ſenſations, or gave a looſe to imagination, becauſe I was rather prejudiced againſt the inflammable air, after I had ſeen various animals immediately die in it, and I was rather fearful when I firſt began to breathe it: beſides, this facility of breathing it, accompanied with a pleaſing ſenſation, I have conſtantly obſerved in all my experiments upon this ſubject.

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This pleaſure, however, I paid very dear for in another experiment, in which I was near loſing my life. Having filled a bladder of the largeſt ſort with about 350 cubic inches of inflammable air extracted from iron filings through water, which air was not at all diminiſhed by the mixture of nitrous air; I began to breathe it boldly (owing to the encouragement received from the above related experiment), and reſolved to breathe it as long as my ſtrength would permit me, after having made a very violent expiration in order to evacuate the lungs of the atmoſpheric air. Having made the firſt inſpiration I felt a great oppreſſion upon my lungs. Towards the middle of the ſecond inſpiration I heard Mr. CAVALLO, who favoured me with his aſſiſtance in theſe experiments, ſay, that I was become very pale: by this time the objects appeared confuſed to my eyes. Notwithſtanding this, I made the third inſpiration; but now my ſtrength failing; I loſt my ſight intirely, and fell upon my knees. In this ſituation I breathed the air of the room, but my knees not being able to ſupport me, I fell intirely upon the floor. However, in a ſhort time I came to myſelf, ſo as to be able to get up, &c.; but my reſpiration continued to be affected with difficulty and pain, as if I had a great weight upon the breaſt; nor did I perfectly recover before the next day.

It muſt be obſerved, that during this experiment I kept my noſe cloſe ſtopped.

This ſame inflammable air contained in the bladder, which I had breathed three times, was examined in various manners, and was found to be as inflammable as before; it exploded as uſual, when mixed with dephlogiſticated air, but after having been ſhaken in water for a ſhort time, being tried with the nitrous air, it gave III−10, IV−10, whereas before it was not at all diminiſhed. At this time the common air, with the ſame nitrous air, gave II−14, II+10. Hence it appears, that the inflammable air, after being breathed, is rather better than before, becauſe in that caſe it is a little diminiſhed by the addition of nitrous air.

In order to aſcertain whether this alteration was occaſioned by the bladder or no, I made the following experiment, which, having been often repeated, was conſtantly attended with the ſame event. I introduced into a bladder, which was ſometimes moiſt and ſometimes dry, a quantity of inflammable air, extracted as well from zinc as from iron, through water, and having kept it in that ſituation for ſome minutes, beating in the mean while the bladder, to keep the air in agitation, I afterwards took it out, and by the mixture of nitrous air ob-

ſerved, that it ſuffered no diminution, exactly as it ſuffered none before it had been put into the bladder.

Having aſcertained, in this manner, that the bladders do not in any manner contribute to render the inflammable air extracted from metals better in its nature, there remained no other way of aſcertaining Mr. SHEELE's experiments, and of underſtanding why I had been able to breathe it eleven times, than by ſuppoſing that the air of the lungs, which can never be thoroughly emptied by being mixed with the inflammable air, alters it, &c. It is well known, that in an ordinary expiration about thirty-five cubic inches of air are expelled from the lungs. In a very violent expiration, following a natural inſpiration, the air expelled may amount to ſixty cubic inches. Theſe forty inches of pulmonary air are mixed with the inflammable air, and are expelled from the lungs in proportion to the remaining air that is breathed after that the lungs have been thoroughly emptied. In the expement above related, of the three inſpirations I made into the inflammable air, it may be eaſily ſuppoſed, that twenty inches or more of pulmonary air were joined with the inflammable air, and entered into the bladder. This pulmonary air, although it is itſelf partly phlogiſticated, is however diminiſhed by nitrous air; and when it ſtands in the bladder it is nearly equal to $\frac{1}{17}$th of the inflammable

air of the bladder breathed three times; hence this loſt ten parts by the mixture of nitrous air.

This explanation, which it is neceſſary to adopt after having exploded all the other hypotheſes, is very analogous to the above related experiment of the ſmaller bladder filled with inflammable air which was breathed eleven times ſucceſſively. This air was breathed after a natural expiration, ſo that there ſtill remained in the lungs about ſeventy-five inches of common air. Theſe ſeventy-five inches of pulmonary air, together with the eighty inches of inflammable air, were mixed together during the eleven inſpirations and expirations; hence the air of the bladder was a mixture of nearly equal portions of inflammable and common air; and, accordingly, when tried with the nitrous air, it was found to be much better (though it had been breathed eleven times) than the air of the large bladder breathed three times only, after the lungs had been emptied as much as poſſible.

All the other experiments that I have made in confirmation of this hypotheſis ſeem univerſally to favour it. If a Guinea pig is introduced into a receiver containing 400 cubic inches of inflammable air, or a ſmall bird into only fifty inches of it, and they be left therein till they are dead, that air afterwards will not be ſenſibly diminiſhed

niſhed by nitrous air; but if a much larger animal is introduced into the 400 inches of inflammable air, or a ſmall animal into a few cubic inches of that air, then it will be found to be ſenſibly diminiſhed by nitrous air; and this diminution will be greater as the animal is larger in proportion to the quantity of inflammable air. A larger animal imparts a greater quantity of its pulmonary air to the inflammable air; and the inflammable air will be found joined to a quantity of pulmonary air, which is ſo much the leſs as the animal is ſmaller.

Mr. SHEELE ſays, he found that the inflammable air after being breathed ſome time intirely loſes its inflammability; from whence he concludes, that the lungs, inſtead of imparting ſome phlogiſton to, imbibe it from, whatever ſubſtance it can be extracted. Though all the direct experiments which ſhew that a phlogiſtic principle is continually detached from the lungs, and joins itſelf to the common air, were wanting, ſtill Mr. SHEELE's conſequence could not be drawn, becauſe the experiment is not true. With reſpect to my own experience I may ſafely ſay, that I have always found it inflammable in every circumſtance, even after I had breathed it eleven times ſucceſſively: and I have not only found it inflammable in the bladder, but I have fired it in the act of let-

ting it out of my mouth. In this manner a flame may be produced from the mouth, of various inches in length, and two or more inches in breadth.

But whence does that ſenſation of levity and facility of breathing the inflammable air, which I have deſcribed above, originate? At preſent I can only have recourſe to a mere mechanical cauſe for a ſolution, for I do not obſerve in inflammable air any property that ſeems capable of altering the lungs upon a chemical principle; neither have I obſerved any decompoſition of air, or alteration of the fluids of the animal. It has been obſerved, that inflammable air, after being breathed, comes out of the lungs with the ſame properties it had before. It is alſo known, that inflammable air is not ſenſibly abſorbed by water, at leaſt after a ſhort time. The lungs, or more properly the pulmonary veſicles, are continually moiſtened with fluids; but that air cannot be abſorbed by them, except it be firſt decompoſed. Nothing elſe therefore remains to which we can have recourſe for an explanation of the above mentioned ſenſations, but the well known levity of the inflammable air compared with common air. And indeed the ſenſation I felt when I breathed that air, is like that of a very light fluid which does not oppreſs the lungs, and is hardly felt. This explanation agrees exactly with

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ſome experiments I have made with common air rendered more light by fire. This air I have found may be breathed much eaſier, although not for ſo long a time, as when it is more condenſed. It muſt be ſaid, indeed, that this is occaſioned by another particular cauſe, which has nothing to do with the caſe of the inflammable air, and which cannot be properly examined in this place.

After all, it ſtill remains to be known, why inflammable air, which kills animals ſo ſoon, may be breathed without any oppreſſion, when in a ſmall quantity, *viz.* when it is mixed with common air; and the following experiments, which are very analogous to thoſe related above, will ſhew that the queſtion is not unintereſting.

I introduced 350 cubic inches of common air into a bladder, and after having made as ſtrong an expiration as I could, I applied the neck of the bladder to my mouth, and breathed the air it contained forty times ſucceſſively. Afterwards, having taken the air out of the bladder, I found that it extinguiſhed a light ſeveral times ſucceſſively. It formed various cryſtals with the oil of tartar, but after a very conſiderable time; ſome of theſe cryſtals had the ſhape of needles, others were like flowers: being tried with the nitrous air, it gave II−18, III+18. This air, therefore, was very much phlogiſticated, nor could I poſſibly have breathed it longer than I did, without

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out falling upon the ground, as I already felt my ſtrength failing, and the objects appeared confuſed before my eyes. Into ten cubic inches of this air I introduced a ſmall bird, which, as ſoon as it began to breathe it, made various contorſions with its body, and ſeemed to ſuffer a great deal. It died in ten minutes time; whereas another little bird introduced into a like quantity, that is, into ten cubic inches of common air, lived in it fifty-two minutes, nor did it ſhew any ſign of uneaſineſs before it had been in five minutes.

It remains to be accounted for, why the bird could breathe for five minutes longer in the air of the bladder that a man could. It will be ſufficient to conſider, that when a man in this experiment has made the laſt expiration into the bladder, he is in a ſtate of pain, and his lungs are loaden with a ſuperfluous quantity of phlogiſton, which is not communicated to the air of the bladder; whereas nothing of this takes place with the bird, which, beſides its being in vigour, has a quantity of common air in its lungs. This ſeems confirmed by an experiment, which admits of no doubt. Having breathed the air of the bladder as long as I could, I ſtopped the neck of the bladder with my finger, then breathed the common air ſeveral times; and afterwards putting the neck of the

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ſame bladder to my mouth again, I breathed that very ſame air four times ſucceſſively. Now there is no doubt but that a bird could have breathed it much longer: the reaſon of which diverſity ſeems to be the following, *viz* that a ſmall bird is in want of a ſmall quantity of air for every time it breathes, whereas a man is in want of a much greater quantity; hence the air is rendered more eaſily noxious, and unfit for reſpiration. From all which it may be concluded, that we are in want of a certain quantity of common air neceſſary for reſpiration, and for the ſupport of life; and that this air, after being inſpired, comes out of the lungs leſs fit to be breathed a ſecond time.

It has been obſerved, that the inflammable air cannot be breathed when the lungs are emptied of common air as much as poſſible; but that it may be breathed when the lungs are in a natural ſtate, in which ſtate a quantity of common air, equal to about forty cubic inches, is known to exiſt in the lungs of an adult perſon. This pulmonary air is not infected ſo far as to be incapable of being breathed various times, and of ſupporting life. After having made a natural expiration I have with force expelled from my lungs about thirty inches of air into an empty bladder; and this pulmonary air I have generally been able to breathe eight times ſucceſſively,

but never longer. It is true, however, that I breathed it with ſome oppreſſion, even from the beginning, which does not happen when the inflammable air contained in a bladder is breathed, the lungs being in a natural ſtate.

And now it ſeems no longer difficult to give an anſwer to the queſtion propoſed above, and to account for the ſmall difference obſerved in the breathing of the two different kinds of air in the bladders. The inflammable air, when joined to a great quantity of common air, may be breathed ſafely, becauſe there is a quantity of common air ſufficient for various inſpirations, and that the mixture of the two airs may be breathed till this common air is thoroughly infected. But the inflammable air itſelf is not altered nor decompoſed by the reſpiration. Wherefore we muſt conclude, that the inflammable air is not ſuch a kind of air as can by itſelf alone be directly uſeful for reſpiration. It muſt rather be conſidered as if there was nothing of that air in the caſe of the bladder; and indeed it is found by experience, that the pulmonary air itſelf may be breathed eight or nine times in an empty bladder. The not being able to breathe it eleven times ſucceſſively, as was done when there was inflammable air in the bladder, and the feeling an oppreſſion in the firſt caſe and not in the ſecond, muſt be intirely attributed to the want of thirty-five cubic inches of air

expirated,

expirated, which are neceſſary to give the lungs all the neceſſary expanſion; whereas, in the other caſe, the inflammable air ſerves to fill up ſpace, and, together with the common air, contributes to fill the lungs; ſo that the inflammable air, conſidered under theſe circumſtances, and under this point of view, may be ſaid to be uſeful for animal reſpiration. This explanation ſeems moſt evidently demonſtrated by the following experiment. If thirty-five cubic inches of common air are introduced into the bladder, and this air be breathed when the lungs are in a natural ſtate, it will be found, that one may breathe it for twenty times or longer; whereas, when the bladder was empty, it could not be breathed more than nine times at moſt.

Before I finiſh this paper it will be proper to mention another cauſe, which, perhaps, alſo contributes to render the inflammable air of the bladder leſs noxious: this is the levity of the inflammable air itſelf with reſpect to common air, which hinders the inflammable mixing with the common air. The inflammable air ſwims continually upon the common air, juſt as æther ſwims upon water; and the inflammable air ſwims ſtill better than æther, becauſe it is much lighter in compariſon than æther. Various experiments made upon volatile ſubſtances have convinced me of this truth. If equal quan-

tities of common and inflammable air, or dephlogisticated and inflammable air, are put into a tube, and two birds are introduced in it, ſo that one of them may ſtand at the top, and the other at the lower part of the inverted jar; it will be found, that the firſt mentioned of theſe birds not only will die conſiderably ſooner than the other, but will ſhew ſigns of uneaſineſs as ſoon as it is come to that place.

The inflammable air, therefore, when breathed together with a conſiderable quantity of common air, muſt always ſwim at the top of it, filling the cavity of the wind-pipe, &c. while the common air occupies the lower place, and filling the ſmalleſt pulmonary veſicles is ſubſervient to the ordinary functions of the lungs.

Here I put an end to my obſervations upon inflammable air conſidered with reſpect to reſpiration; but I beg leave to add a few words reſpecting a property of the inflammable air, which, as far as I know, has been overlooked by the moſt diligent obſervers.

I mean here to ſpeak of ſuch inflammable air as is extracted from metals, by means of oil of vitriol, eſpecially that extracted from iron and zinc. The air of theſe metals, when preſented to the flame of a candle, not only burns with a whitiſh flame inclining to green (as is well known); but exhibits a kind of ſparks or exploſions

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high may be easily distinguished between the body of the flame by their vivid light. These sparks, which are of a vivid colour, dart in every direction. They might be easily taken for those sparks that are emitted from red-hot iron; or they might be compared to very small grains of gunpowder, if these were inflamed successively, and without smoke; or they might even be compared to charcoal that sparkles, but without any noise. This phenomenon seems very interesting, as it respects the nature of the inflammable air itself. What seems to me most singular is, that this appearance forms a distinctive character between the inflammable air of metals, and that extracted from animal or vegetable substances; at least I may safely say, that I never found the inflammable air of animal or vegetable substances sparkle like that extracted from metals. In several of the former kinds of air I could observe no sparkling at all; in others the sparks were so few that they might be considered as nothing in comparison to the sparkling of the inflammable air from metals.

The inflammable air of metals itself, if left in contact with water for a long time, or shook in it till it becomes less inflammable, will in great measure lose its sparkling property, and at last loses it intirely, when it is become in a state of being hardly inflammable. I have observed,

that the inflammable air is the more difficult to be decompoſed, by being ſhaken in water, as the number of the ſparks it ſhews when burning is greater; and according to this number of ſparks, the inflammable air makes greater or weaker explosions when mixed with the dephlogiſticated air; ſo that it ſeems proved by experiments, that the phlogiſtic principle is more fixed and in greater quantity combined with the inflammable air of metals, than with that of vegetable or animal ſubſtances. I do not mean to deny the poſſibility of finding other ſpecies of inflammable air extracted from other ſubſtances beſides metals which may explode like that extracted from metals; but I only ſay, that in thoſe caſes the inflammable air will alſo ſparkle more, and will be found leſs eaſy to be decompoſed by water. There are other ſubſtances that give the inflammable air in great quantity, and which cannot be conſidered as animal or vegetable ſubſtances, but come rather near the nature of metals; as, for inſtance, the ſpathoſe iron, from which I extract a good deal of inflammable air by the action of fire only, applied to a mattraſs. But the metal in this ſubſtance is not in its pure ſtate, and it may be conſidered rather as a calx of iron than true iron. Accordingly, this air can hardly ſparkle at all; it explodes more like

the inflammable air of vegetable or animal bodies than that of metals, and it is eafily decompofed in water.

This property of inflammable air of metals which I have difcovered, throws great light upon the analyfis of the decompofition of that air which I have made in two different ways. The firft is to fire it together with common or dephlogifticated air, in veffels filled with very pure quickfilver, and alfo in veffels filled with diftilled water. The fecond method is to decompofe it by fhaking it in pure diftilled water. In the firft procefs a great number of experiments are required in order to obtain a fenfible reſiduum; befides, the igneous part is loft. The fecond method requires an exceedingly long time, but it is the moft complete; for which reafon I have ufed it for the decompofition of other kinds of air.

XXV. *On the Variation of the Temperature of boiling Water.* By Sir George Shuckburgh, *Baronet, F. R. and A. S. and Member of the Academy of Sciences and Belles Lettres at* Lyon.

Read March 11, 1779.

THE heat of boiling water having for ſome years been uſed as one of the terms for graduating the ſcale of thermometers; together with the particular attention the Society has lately given (*vide* the Report of the Committee, Phil. Tranſ. vol. LXVII.) to this branch of inquiry, and I may add the ſingular ſucceſs with which this age and nation has introduced a mathematical preciſion, hitherto unheard of, into the conſtruction of philoſophical inſtruments, will render it unneceſſary for me to ſay more in reſpect of the following experiments, than ſimply to lay them before the Royal Society.

That the heat of boiling water was variable, according to the preſſure of the atmoſphere, ſeems to have been known to FAHRENHEIT as early as the year 1724[a].

(a) *Vide* Phil. Tranſ. N° 385. wherein is propoſed a curious project of determining the weight of the atmoſphere by means of a thermometer alone, under the title of "Barometri novi deſcriptio."

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A few years after this, Meſſieurs LE MONNIER and CASSINI (*Mem. de l'Acad. des Sc. pour* 1740) made ſome deciſive obſervations, to ſhew that this quantity was very conſiderable. It was left, however, for Mr. DE LUC to make a much more compleat ſeries of experiments, which he has deſcribed and reduced into ſyſtem in his *Recherches ſur la Variation de la Chaleur de l'Eau bouillante*. It remained only that theſe ſhould be verified. Towards the latter end of the year 1775 I had an opportunity of repeating theſe obſervations with a ſmall pocket thermometer of about ſix inches long, made by Mr. NAIRNE; an inſtrument, it muſt be confeſſed, not very accurate for ſuch an examination, but with which I thought, however, I could obſerve to within a quarter of a degree; my object at that time, amidſt a variety of other philoſophical purſuits, being to aſſure myſelf that the variation took place, rather than critically to examine the quantity of it. I ſhall relate theſe obſervations, as the reſult of them upon my return to England led me to ſome more accurate.

Table of obſervations of the boiling-point, made in a journey over the Alps.

Place of obſervation.	Height of the barometer.	Heat of boiling water by obſervat.	Heat of boiling water by Mr. DE LUC's rule thermometer	Diff.	Difference in $\frac{1}{100}$ of total.
	inches.	°	°	°	
Bologna,	30.11	213.5	213.5	0.0	0
Geneva,	28.60	210.4	210.9	−0,5	− 16
Modane,	26.61	207.3	207.4	−0,1	− 2
Lannebourg,	25.75	205.1	205.9	−0,8	− 9
Mount Cenis,	24.03	201.2	202.6	−1,4	−11
Ditto,	23.91	201.1	202.4	−1,3	−10
			Mean	$\frac{9}{100}$ =	$\frac{1}{11}$

The ſecond column gives the height of the barometer at the time of obſervation; the fourth, the heat of boiling water deduced from Mr. DE LUC's rules, compared with the lowermoſt obſervation, or that under the greateſt preſſure; the ſixth gives the difference between the theory and the experiment in the motion of the boiling point in hundredth parts of the whole ſpace deſcribed: from whence it might be concluded, that the motion or variation of the boiling point with a given variation in the preſſure of the atmoſphere was $\frac{9}{100}$ or $\frac{1}{11}$ greater

greater than by the theory alluded to [b]. But theſe were but groſs experiments, and perhaps unworthy of ſuch a competition. They induced me, however, to make the following. In the beginning of laſt year (1778) with the aſſiſtance of Mr. RAMSDEN, I procured a moſt excellent thermometer, every way adapted for this purpoſe. It was about fourteen inches long, but the interval between freezing and boiling only $8\frac{1}{4}$ inches [c], and though every degree was ſomething leſs than the $\frac{1}{10}$th of an inch, yet, by means of a ſemi-tranſparent piece of ivory, which applied itſelf cloſe behind the glaſs tube, ſliding up and down in a groove cut in the braſs ſcale for that purpoſe, carrying a hair-line diviſion, at the extremity of which was a vernier dividing each de-

(b) The ſame inſtrument, immerſed in ſnow juſt melting at the top of Mount Cenis fell to 32°, the point of freezing obſerved at the level of the ſea.

(c) It may poſſibly be ſuggeſted, that if this interval had been greater, *viz.* 20, 30, or 40 inches, I ſhould have had a much larger ſcale and more convenient inſtrument; but in this, as in moſt other mechanical contrivances, our progreſs beyond certain limits is prevented; for if the perpendicular height of the column of quickſilver be much increaſed, the weight of it will be ſuch as to diſtend the ball, and the inſtrument may differ from itſelf in a vertical and horizontal poſition by half a degree, as I have ſeen in a tube only fifteen inches long; and if this circumſtance be endeavoured to be corrected by making the bulb of the thermometer thicker, its ſenſibility will be proportionably diminiſhed. If my experience were to lead me to conclude any thing, I ſhould conſider a tube of a foot long as a *maximum*, and the bore of ſuch a diameter as to admit a ball of a quarter or one fifth of an inch.

green into ten; with, moreover, a lens of an inch focus; this apparatus being made moveable first by the hand, and more delicately by means of a micrometer screw, whose head was divided into twenty-five divisions, each equal to the fortieth of a degree (for so truly cylindrical was the tube, which had been with care expressly selected from a great quantity of glass, that the divisions in the neighbourhood of the freezing point did not differ from those near the boiling point by so much as $\frac{1}{40}$th of a degree, and this variation appeared in other parts of the tube strictly uniform, as was found by breaking the column of mercury); by means I say of this apparatus I was enabled to read off any height of the thermometer to within $\frac{1}{50}$th of a degree. The vessel, in which the water was boiled, which was always spring water, was a cylindrical tin pot, 13 inches high and $4\frac{1}{4}$ inches wide, with a top something resembling that described in Mr. DE LUC's work, contrived to carry off the steam without incommoding the observer, with a waste pipe for the superfluous water in boiling, which might otherwise fall upon the fire and extinguish it. The ball of the thermometer was immersed to within $2\frac{1}{2}$ inches of the bottom of the vessel, and $10\frac{1}{2}$ inches below the surface of the water, so that as near as might be the whole column of mercury was exposed to the heat of boiling water,

there

there being only $1\frac{5}{50.0}$ of 20° of the ſcale, equal about part riſing out of the water expoſed to the temperature of the ſteam, which in one or two experiments was found to be 180° or 190°, ſo that the correction for this defect of heat would only amount to a very few hundredths of a degree, perhaps about ,04 or 05, which, as the inſtrument was expoſed to the ſame circumſtances as near as might be in all the obſervations, I have taken no notice of. I thought it neceſſary to ſay thus much reſpecting the preciſion of the inſtrument and the apparatus, and ſhall now relate the obſervations at length.

Table of the obſervations.

No.	Place of obſervation.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
1	Summit of Snowdon in Carnarvonſhire, Aug. 15, 1778,	26,487	46	26,498	207, 8 206,38	207,07
2	Top of Carn Cwm Gafr, Aug. 16, 1778,	27,274	62	27,241	208,27 208,24 208,28 208,19 208,29	208,64
3	In the deſcent from ditto,	27,957	51	27,954	209,28½ 209,37 209,39 209,32 209,32 209,40	209,87

Table of observations continued.

Number.	Place of Observations.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
4	Shuckburgh, Dec. 3, 1778,	28,360	44	28,377	210,17½ 210,20½ 210,20½ 210,21½	210,50
5	Ditto, Oct. 23, 1778,	28,690	47	28,699	211, 8 211,10 211,11 211,13 211,12 211,10	211,27
6	Ditto, Sept. 27, 1778,	28,910	54	28,898	211,19½ 211,20½ 211,24½ 211,17½ 211,14½	211,50
7	Ditto, Nov. 7, 1778,	18,990	47	28,999	211,23 211,24 211,25 211,23	211,60
8	Llanberis, Carnarvonshire, Aug. 13, 1778,	29,477	60	29,447	212,22	212,55
9	London, Jan. 27, 1779,	29,790	45	29,805	212,35 212,40 212,42 212,36 212,36 212,38 212,39	212,95

Table

Table of Observations continued.

Number.	Place of observation.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
10	London, Jan. 28, 1779,	29,990	44	30,008	213, 5	
					213, 8	
					213,10	
					213, 9	
					213,18	
					213,10	213,22
					213, 6	
					213, 6	
					213,11	
					213,12	
					213, 8	
11	Ditto, June 10, 1778,	30,250	64	30,207	213,20	
					213,23	
					213,24	213,58
					213,26	
					213,23½	
12	Ditto, Dec. 27, 1778,	30,468	43	30,489	214, 5	
					214, 9	
					214, 6	
					214, 2	
					214, 5	214,15
					214, 7	
					214, 7	
					214, 5	
13	Ditto, Dec. 24, 1778,	30,750	46	30,763	214,18	
					214,12	214,37
					214,15	

Table

Table of obſervations continued.

Number.	Place of obſervation.	Height of the barometer.	Therm. ſt.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
14	London, Dec. 25, 1778,	30,838	47	30,847	214,35 214,32 214,33 214,34 214,35 214,31 214,33	214,83
15	Ditto, at the Adelphi Wharf, 3½ feet above high water, Dec. 26, 1778,	30,948(*d*)	47	30,957	214,35 214,39 214,40 214,38½ 214,41 214,37 214,41 214,40 214,40 214,34	214,15

The numbers in the fifth column are the corrected heights of the barometer reduced to one and the ſame temperature, *viz.* 50°, which was neceſſary in order to have the true proportion of the preſſure of the atmoſphere, whoſe influence ſeems to have ſo conſiderable a

(*d*) This is the greateſt height of the barometer that I have ever known, and, as far as I have been able to collect the higheſt point, that it has ever been ſeen to ſtand at in any country where obſervations have been made and recorded, ſince the firſt invention of this ingenious inſtrument.

ſhare

ſhare in the heat of boiling water. Column the ſixth ſhews the height the thermometer ſtood at in water boiling very faſt in degrees and 40ths of a degree; where it will be ſeen, that the obſervation was repeated, as the heat is given of every ſeparate trial, which is, perhaps, the beſt criterion of the confidence that is to be placed in the mean reſult, ſhewn in column the ſeventh expreſſed in hundredths of a degree.

Having collected this ſeries of experiments, I was anxious to ſee how far they correſponded with Mr. DE LUC's, and upon compariſon of N° 1. and N° 15. I found that the decreaſe of the boiling heat was $\frac{,47}{100}$ greater than the rules admitted of from an alteration of the preſſure of the atmoſphere of $4\frac{1}{2}$ inches. This difference led me into an examination of all my obſervations, to ſee how far they were conſiſtent with themſelves; how far they diſagreed from Mr. DE LUC's; and, laſtly, what general concluſion might be drawn from them.

To avoid in ſome meaſure, or at leaſt correct, the errors of obſervation, the mean of N° 1. and N° 2. the mean of N° 6. and N° 7. and of N° 14. and N° 15. was taken inſtead of either obſervation ſeparately; the firſt and third of theſe means as two extreme terms, and the ſecond as an intermediate one: with theſe it was very eaſy by interpolation or proportion to deduce any other

intermediate term, and conſequently from the mean of ſix to examine all the fifteen obſervations. Thus, on a compariſon of N° 1. and 2. with N° 14. and 15. the mean motion of the boiling point in that interval for one inch of the barometer (*viz.* when the mercury ſtands at 28,886 inches) is = 1°,743; according to Mr. DE LUC this is 1°,65. By a ſimilar compariſon of N° 1. and 2. with N° 6. and 7. the mean motion of the boiling point in that interval (*viz.* at 27,908 inches) for one inch is = 1°,779; by Mr. DE LUC = 1°,73. And, laſtly, comparing the mean of N° 6. and 7. with the mean of N° 14. and 15. the mean motion of the boiling point in this interval (*viz.* at 29,925 inches) for one inch is = 1°,709; by Mr. DE LUC = 1°,59. It ſhould follow then, that, within the limits of my experiments, the alteration or motion of the boiling point is greater [e] by $\frac{1}{18}$ than from that gentleman's obſervations, that the heat of boiling water is not directly in the ſimple ratio of the height of the barometer, nor yet is the progreſſion ſo rapid as Mr. DE LUC obſerved it. It may be ſomewhat ſatisfactory to ſee the obſervations collated and compared.

(e) It is true, that my obſervations in Savoy give this difference $+\frac{1}{11}$ thoſe of Mr. LE MONNIER equal $+\frac{1}{12}$ (Vide *Recherches ſur l'At.* § 964.); and though, perhaps, neither the one nor the other are intirely unexceptionable, they tend, however, ſomething to confirm, although alone they may be unable wholly to ſupport, ſuch a ſuppoſition.

N°

N°	Height of the barometer.	Boiling point by observation.	Boiling pt. by Mr. DE LUC's rules.	Error.	Boiling point by interpolation from N° 1, 2, 6, 7, 14, and 15.	Error.
M	Inch.	°	°	°	°	°
1	26,498	207,07	208,54	+0,47	207,18	+0,11
2	27,241	208,64	208,84	+0,20	208,52	−0,12
3	27,954	209,87	210,03	+0,16	209,80	−0, 7
4	28,377	210,50	210,81	+0,31	210,56	+0, 6
5	28,699	211,27	211,34	+0, 7	211,11	−0,16
6	28,898	211,50	211,67	+0,17	211,47	−0, 3
7	28,999	211,60	211,85	+0,25	211,64	+0, 4
8	29,447	212,55	212,74	+0,19	212,42	−0,13
9	29,805	212,95	213,15	+0,20	213,03	+0, 8
10	30,008	213,22	213,47	+0,25	213,49	+0,27
11	30,207	213,58	213,79	+0,21	213,71	+0,13
12	30,489	214,15	214,23	+0, 8	214,19	+0, 4
13	30,763	214,37	214,66	+0,29	214,65	+0,28
14	30,847	214,83	214,79	−0, 4	214,79	−0, 4
15	30,957	214,96	214,96	,0	214,96	+,0,2

I shall make no deductions from this comparison, but leave them to the leisure of the reader. It will, however, probably be inquired, how the thermometer came to stand at 213° in boiling water, when the barometer was about 30 inches, 212° being the degree by which that heat is expressed on FAHRENHEIT's scale? The answer is easy: it was an error in the making of the instrument, and, I believe, a pretty general one. There was

alſo a ſimilar error of ſomething more than one quarter of a degree in laying down the freezing point ſo that the fundamental interval between freezing and boiling, when the barometer ſtands at 29,8 inches (the mean height at London) was 180°,71 inſtead of 180°; by this means each diviſion was in fact $\frac{1}{260}$th part leſs than a degree: this ſmall correction may therefore eaſily be applied, if thought neceſſary, but I have taken no notice of it.

I will now add a general table for the uſe of artiſts in making this inſtrument, both according to my own obſervations, and thoſe of Mr. DE LUC, that the preference may be given as it ſhall be thought due; not that it is a matter of any great conſequence which is made uſe of under ſmall variations of the atmoſphere; but even under theſe circumſtances, I flatter myſelf, that the object of this paper will be ſufficiently obvious to all who wiſh to verify a new theory, or aim at accuracy in theſe days of preciſion.

Height

Height of the barom.	Correct. of the boiling point.	Diff.	Correct. according to Mr. DE LUC.	Diff.
26,0	−7,09		−6,83	
		,91		,90
26,5	−6,18		−5,93	
		,91		,89
27,0	−5,27		−5,04	
		,90		,88
27,5	−4,37		−4,16	
		,89		,87
28,0	−3,48		−3,31	
		,89		,86
28,5	−2,59		−2,45	
		,87		,83
29,0	−1,72		−1,62	
		,87		,82
29,5	−0,85		−0,80	
		,85		,80
30,0	0,00		−0,00	
		,85		,79
30,5	+0,85		+0,79	
		,84		,78
31,0	+1,69		+1,57	

XXVI. *Account of a new Kind of inflammable Air or Gass, which can be made in a Moment without Apparatus, and is as fit for Explosion as other inflammable Gasses in use for that Purpose; together with a new Theory of Gunpowder.* By John Ingen-Housz, *Body Physician to their Imperial Majesties, and F. R. S.*

Read March 25, 1779.

THE important discoveries on different kinds of air have opened a new field for one of the most pleasing and interesting scenes which ever were exposed to the contemplation of philosophers, and therefore could not fail of exciting in almost every lover of natural knowledge a decided curiosity to see the pursuit of such striking novelties, and in many an almost irresistable temptation to imitate them, and to pursue farther the road already so far opened by the Rev. Dr. PRIESTLEY, Abbé FONTANA, Mr. LAVOISIER, and many other learned and ingenious men.

Who can, indeed, without the greatest satisfaction, look (amidst many other objects of admiration) upon the discovery

diſcovery of that new aerial fluid, which in purity and fitneſs for reſpiration ſo far ſurpaſſes the beſt atmoſpherical air, that an animal protracts his life five times as long, or even more, in it than in common air of the beſt quality?

Dr. PRIESTLEY, the firſt who diſcovered this wonderful fluid, extracted it from bodies which we ſhould rather have ſuſpected to have contained deleterious exhalations. He afterwards found it exiſted in many other bodies in which the moſt accurate obſerver never found any thing of an approaching nature.

When I conſider the immenſe quantity of this pure aerial fluid, called by Dr. PRIESTLEY with ſo much propriety *dephlogiſticated air*, which exiſts as it were in a ſolid ſtate in nitre, in calcined metals, and even in ſome other of the moſt common ſubſtances, I cannot expreſs the greatneſs of my expectation, as a phyſician, from ſuch an important diſcovery [a]. I flatter myſelf, that ere long an eaſy and cheap method will be diſcovered by which ſuch quantities of this beneficial air may be ob-

(a) Since Dr. PRIESTLEY's laſt publication on air, he diſcovered, that the ſame water which, if examined immediately, gives only a ſmall quantity of bad air, yields ſpontaneouſly about ten times the quantity of pure dephlogiſticated air after ſtanding ſome time expoſed to the ſun. Of this I was an eye-witneſs. The important diſcoveries of Abbé FONTANA upon this ſubject, which he ſhewed me when I was laſt in Paris a year and a half ago, will ſoon be publiſhed by himſelf. He extracted this wonderful aerial fluid from different kinds of water by boiling them over a fire.

tained as will ſerve to cure ſeveral diſeaſes which reſiſt the power of all other remedies, and ſo prolong, as it were, human life. We may expect, with ſome degree of confidence, that this new element, when it ſhall be uſed for the benefit of reſpiration, will be found more fit than the beſt common air to free our body from that quantity of phlogiſton or inflammable principle which ſeems to exiſt ſometimes in too great a quantity in the maſs of our blood; or from which it ſeems ſometimes, as it were, to be let looſe in too great abundance, producing, perhaps, in conſequence fevers and other ſymptoms, the cauſes of which have not yet been clearly elucidated by the beſt medical writers.

This dephlogiſticated air, free from the inflammable particles with which the beſt common air is always infected, will probably be found capable of abſorbing a greater quantity of thoſe phlogiſtic particles with which the air coming from our lungs is always found to be pregnant, and thus of ventilating, as it were, much more expeditiouſly the maſs of our blood of that which a conſtant exertion of the organs of reſpiration is not always able to free it from in a ſufficient quantity.

Theſe important purſuits have led Dr. PRIESTLEY to the diſcovery of one of the benefits, and perhaps the principal, we derive from reſpiration; that function of the

animal oeconomy which is ſo important, that without its conſtant influence an animal, once born, has but a few moments to live.

The criterion of the degree of goodneſs of reſpirable air, by the quantity which is abſorbed or deſtroyed by the addition of nitrous air, is one of thoſe uſeful diſcoveries of Dr. PRIESTLEY's from which mankind will, perhaps, hereafter reap a conſiderable benefit.

The diſcovery of the various kinds of inflammable airs or gaſſes becoming powerfully exploſive, when they are mixed with a ſufficient quantity of common air, and ſtill more ſo when they are combined with dephlogiſticated air, is one of thoſe improvements in natural philoſophy which, giving occaſion to various amuſing and intereſting experiments, have caſt at the ſame time a new light upon ſome powerful agents, whoſe miſchievous force was known, though their nature was ſtill in the dark.

As thoſe inflammable airs have been of late years one of the principal philoſophical amuſements, I intend to lay before the Royal Society an eaſy method of producing, without any trouble or particular apparatus, ſuch quantity of an inflammable air or gaſs as may be required; to which I ſhall ſubjoin ſome conſiderations which have occurred to me about the nature of that wonderful ſubſtance gunpowder, which conveys death at ſuch an immenſe diſtance by its almoſt irreſiſtible exploſive force,

force, by which it propels metallic balls of a conſiderable weight. To inveſtigate the nature of this awful ingredient, by which the fate of empires is decided in our days, cannot be indifferent to any body who takes delight in inveſtigating natural cauſes.

Being at Amſterdam in November 1777, Meſſieurs AENEAE and CUTHBERTSON, two ingenious philoſophers of that city, were ſo good as to ſhew me ſome curious experiments with exploſive and inflammable airs of different kinds. They produced an inflammable air, by mixing together equal quanties of oil of vitriol and ſpirit of wine, and applying heat to the phial containing the compound. A great quantity of white vapour was extricated, which, paſſing up the inverted receiver filled with water, ſettled at the top and depreſſed the water, as other airs do. This air ſoon became clear, the white fumes being abſorbed by the water. This air was eaſily lighted in an open cylindrical glaſs, and burnt almoſt as clear as a candle, the flame deſcending gradually lower and lower till it reached the bottom. A very little quantity of this air mixed with common or dephlogiſticated air, for inſtance, one fourteenth or one tenth part, and kindled by an electrical ſpark, exploded with a very loud report, and ſhattered the glaſs to pieces in which it was kindled, when it did not find a ready vent.

They

They had contrived a kind of a piſtol for the purpoſe, conſiſting of a ſtrong cylindrical glaſs tube with a piſton adapted to it. To the end of this tube was fixed a braſs barrel, like that of a common piſtol: into this barrel a braſs bullet was put looſe, ſo that the barrel was placed a little above the level, to prevent the bullet rolling out. The barrel was directed to a board of oak at eight or ten feet diſtance. A proper quantity of common and inflammable air (produced in the manner above mentioned) being drawn into the glaſs tube by means of the piſton, it was fired by directing an electrical exploſion through it. The exploſion was very loud: the ball hit the board with ſuch a force that it made a ſtrong impreſſion in it, and recoiled with a conſiderable force, ſo as to hit the wall behind us, and to put us in ſome danger of being hurt by its rebounding force.

The ſame gentlemen told me, that this inflammable air had in ſome reſpects the advantage over the inflammable airs extracted from metals by the vitriolic or marine acid, and that extracted from mud or marſhes; becauſe this air being heavier than either of theſe airs, and even than common air, is not ſo eaſily loſt out of an open veſſel; and, that when it eſcapes into the open air, it agreably perfumes the room with the ſmell of *ſpiritus vitrioli dulcis* or æther; whereas the other inflammable airs,

which from their leſs ſpecific gravity eſcape eaſily into the common air, yield an offenſive, diſagreeable ſtench.

Mr. AENEAE, having examined the ſpecific gravities of the different inflammable airs compared with common air, favoured me with the following reſult of his inquiries.

A veſſel, which contained the weight of 138 grains of common air, contained 25 grains of inflammable air extracted from iron by vitriolic acid, and 92 grains of inflammable air extracted from mud or marſhes, and 150 grains of that extracted from oil of vitriol and ſpirit of wine.

I was much pleaſed with the above mentioned experiment, and immediately thought that the operation of extracting this inflammable air or vapour could be diſpenſed with by employing vitriolic æther, which in reality is contained in the vapour expelled by heat from oil of vitriol and ſpirit of wine, which vapour, condenſed in the proceſs of diſtillation, yields æther. But I reſolved to defer making the experiment till I ſhould arrive in London, where I intended to make ſome ſtay to ſee my old friends, and acquire what medical and philoſophical knowledge I could.

Having arrived in this capital in the beginning of January 1778, I loſt no time in purſuing my idea. For

this purpoſe I poured ſome drops of æther into a ſtrong glaſs tube, and directed an electrical exploſion from a Leyden phial through it; but, to my mortification, no exploſion happened. I then threw a bit of cotton, dipped in æther, into the ſame tube, but it would not take fire. Though theſe firſt trials proved unſucceſsful, I was too much perſuaded, that in ſome way or other it muſt ſucceed, to be diſcouraged. I tried it in ſeveral different ways, and at laſt, before the end of January, I ſucceeded once or twice in producing a loud exploſion, by throwing into the tube a little bit of paper dipped in æther. But as the experiment often failed (of which I could then find no reaſon), I did not venture to ſhew it to my friends till I had hit upon a method, the certainty of which would prevent my being expoſed to ſome confuſion by exhibiting an experiment which was ſo apt to fail. However, I told Sir JOHN PRINGLE, Meſſ. NAIRNE and BLUNT, and ſome few others of my friends, early in the ſpring, that I had found out a method of firing an inflammable air piſtol without being at the trouble of making inflammable air in the ordinary way; as I produced it at pleaſure in an inſtant, without any trouble or apparatus; and that I would ſhew them the experiment as ſoon as I was ſure of ſucceeding conſtantly. In the mean time I continued to produce this air before my acquaintance in

the way I had ſeen it produced at Amſterdam. Soon after, hitting upon better and ſurer methods of ſucceeding, I began to ſhow it to thoſe who came to viſit me, and in the beginning of the ſummer I made no ſcruple of ſhewing it to every body.

The reaſons why I did not ſucceed in the beginning I found afterwards to be, either that I employed too great a quantity of æther, or that the air or vapour of the æther was not thoroughly incorporated with the other air; for the ſame numbr of drops of æther poured into the air piſtol, which would not produce an explofion when the piſtol was not ſhaken, made a very loud one when it was forcibly agitated.

The ſureſt method of ſucceeding I find to be the following: I dip a ſmall glaſs tube, open on both ſides, and the bore of which is one twelfth of an inch in diameter into a phial containing æther, and when two or three drops of the liquid have entered the tube I apply my finger to the upper end of it, to keep the liquor ſuſpended. I take the tube out of the phial, and thruſt it immediately into a ſmall *caoutchouck*, or elaſtic gum bottle: this being done, I withdraw my finger from the tube, and take it out of the *caoutchouck*; thus the little quantity of æther, ſuſpended in the end of the tube, is dropped into the *caoutchouck*, the neck of which is to be

immediately

immediately inverted into the orifice of the air piſtol, and, after giving it a gentle ſqueeze, withdrawn out of it: after which, a bullet or a cork is to be thruſt into the mouth of the piſtol, when it is ready for firing. This whole operation may be performed in the ſpace of five or ſix ſeconds.

The conſiderable force of exploſion, and the loud report of the ordinary inflammable airs, induced Mr. VOLTA, of Como, to believe, that theſe airs might, perhaps, become a ſubſtitute to gunpowder. If this expectation had been well founded, the greateſt *deſideratum* would, I think, have been to find out a way to produce ſuch air at any time without trouble, and to carry it about in as little compaſs as poſſible: which two conditions I ſhould have pretty nearly fulfilled, as all the inflammable air requiſite for the exploſion of the piſtols contrived by Mr. VOLTA is contained in the bulk of one ſingle drop of æther; which drop, poured in the piſtol itſelf, is full ſufficient to produce a very powerful exploſion. But how far this expectation of Mr. VOLTA, as to the uſe of inflammable air in offenſive arms, is well grounded, I will attempt to explain in the ſubſequent part of this paper.

This inflammable air (which, perhaps, might more properly be called vapour, as it is abſorbable by water)

has

has the principal properties of the other inflammable airs, *viz.* it will not inflame but where it is in contact with common or dephlogisticated air; and therefore only takes fire at the top of the veſſel containing it, the flame going gradually downwards till the whole is conſumed, if the veſſel is of a cylindrical form, and pretty wide. If it is diluted with common air, but not ſufficiently, it will burn as other inflammable air without exploding. Being ſufficiently diluted with common air, eſpecially with dephlogiſticated air, it explodes with a very great report and a conſiderable force. It is unfit for reſpiration in a concentrated ſtate, and is as mortal for an animal plunged into it as any other of the inflammable airs; though in a diluted ſtate it ſeems to be rather pleaſant to the organs of reſpiration.

It differs in ſome reſpects from the common inflammable airs; as, for inſtance, it is much heavier, as is already obſerved, than any of the other inflammable airs, and even than common air. It does not inflame or explode with ſo ſmall a ſpark of electrical fire as the other inflammable airs, requiring the exploſion of a Leyden phial to be fired with certainty, though one ſingle inch of coated glaſs will be ſufficient; ſo that a Leyden phial, containing twelve or fourteen ſquare inches of coating, may fire an air piſtol loaded with this kind of air ſeveral

times,

times, if the charge be divided by taking it out with a ſmall glaſs tube, fitted up in the manner I deſcribed in the paper I had the honour of laying before the Royal Society laſt year, and which is inſerted in the ſecond part of the 68th vol. of the Phil. Tranſ.; in which paper I deſcribe a kind of an electrical match to light a candle with. This air being in contact with water is abſorbed by it in a few days, though the water be not ſtirred: and much ſooner loſes its inflammability by contact with water than the other inflammable airs do. However, I found that in ſuch a ſituation this air had not yet loſt all its explofive force in ſix days, though the water in which the cylindrical glaſs, ten inches long and one in diameter, was inverted, after I had poured into it five or ſix drops of æther, had gradually aſcended till, on the third day, it reached to the half of the height of the glaſs: ſo that it ſeems as if theſe drops of æther, by their expanding in the form of air, had forced out half of the common air contained in the glaſs, in the place of which the water afterwards aſcended in proportion as the air or vapour generated by the æther was abſorbed by the water.

That this air is ſpecifically heavier than common air, and does not readily incorporate with it, is eaſily demonſtrated by the following experiment. I poured five or ſix drops of æther into a cylindrical glaſs, ten inches long

and

and one inch in diameter: the æther being ſoon evaporated, I clapped my hand upon the mouth of the glaſs, and inverted it to incorporate the air generated by the æther with the common air; after which I left the glaſs open during half an hour, when I dipped in it a piece of wax taper burning, ſtuck upon a bended wire. As ſoon as the taper reached the brim of the glaſs, a flame burſt out with a weak exploſion. In a quarter of an hour I again thruſt the wax taper into the ſame cylinder, and no flame was obſerved till the wax taper reached the place where the flame ended the firſt time. This ſecond exploſion did not ſet fire to the whole at the bottom of the glaſs. I again waited a quarter of an hour, and then again thruſt the wax taper into it, by which the remainder of this inflammable gaſs, which had remained ſettled all that time at the bottom, was exploded [b].

(b) It is remarkable, that æther, the moſt volatile liquor yet known, and ſo apt to ſpread itſelf through the air by a quick evaporation that a drop of very fine æther, which falls from the height of a few feet, is quite evaporated before it reaches the ground, and no glaſs ſtopper of itſelf is able to keep it from evaporating: it is remarkable, I ſay, that notwithſtanding this extreme volatility, the air or vapour, into which it changes by evaporation, ſhould be ſo far from participating of the ſame volatility, that it may be kept hours together in an open cylindrical veſſel without evaporating, mixing with the common air, or loſing its inflammability; ſo that this ſubſtance ſeems to undergo a ſudden metamorphoſis, and to change in an inſtant from the lighteſt of all liquors to one of the heavieſt of aerial fluids, fixed air, which being one half heavier than common air, according to the experiments of Mr. CAVENDISH, is probably much heavier than this æther air.

I found

I found that æther, in which as much urinous phosphorus is dissolved as will make it luminous in the dark, when some drops are poured upon water; is very brisk in taking fire, when employed for an inflammable air pistol; but that the experiment, when repeated, will be apt to fail, because the phosphoric acid which remains in the pistol, and by its nature attracts the humidity of the atmosphere, will soon fill the inside of the pistol with a coat of moisture, and prevent the electrical spark from kindling the inflammable air.

It appeared, that a little camphire dissolved in æther increases its explosive force, and makes it less apt to fail.

As this inflammable air is heavier than common air, it is clear, that the mouth of the air pistol should be kept upwards at the time of charging it; whereas it is better to invert the pistol when the ordinary inflammable airs are employed, which, being specifically lighter than common air, rise of themselves in the pistol when its mouth is placed inverted upon the orifice of the vessel which contains them.

It is true, that the squeezing the elastic gum bottle, when placed upon the pistol, forces some of the inflammable gas out of it, which is lost in the common air; but notwithstanding this waste, the inflammable air which

remains in the piſtol is ſufficient to produce a loud report, which is all that is required. Indeed, one ſingle drop of the æther could be eaſily ſhaken out of the glaſs tube immediately into the piſtol, without making uſe of the elaſtic gum bottle; but this drop, evaporating into elaſtic air, leaves behind it a good deal of moiſture, whether inherent in the æther itſelf, or attracted from the atmoſphere. This moiſture, in the way I uſe to load the piſtol, remains in the elaſtic gum bottle, which is therefore always found moiſt when the experiment is repeated ſeveral times.

It was, indeed, known before this time, that æther and other volatile inflammable liquors ſpread by evaporating inflammable effluvia through the ſurrounding air, eſpecially when they are heated; and that theſe effluvia have ſometimes by the imprudent approach of a candle taken fire, and conveyed the inflammation to the liquor itſelf: but I never heard that any body employed theſe liquors inſtead of ordinary inflammable air in communicating to common air an exploſive quality, or in firing inflammable air piſtols, before I communicated the experiment to my friends

As it will, I think, appear very probable, by what will be ſaid hereafter, that little more than a pleaſing amuſement can be expected from the force of any inflammable air;

air; the ready production of ſuch inflammable air always ready for uſe, when an exploſion is intended to be produced, may be of ſome importance to philoſophers whoſe time muſt be ſparingly taken up.

Comparative view of the expanding force of exploſive air and gunpowder.

The force of gunpowder has been aſcribed by Sir ISAAC NEWTON and all other philoſophers to the ſudden extrication of an amazing quantity of elaſtic permanent aerial fluid within a narrow ſpace incapable of containing it; which quantity ought to be attentively attended to, in order to eſtimate the comparative power of the two ſubſtances.

BENJAMIN ROBINS, whoſe work, intitled, *New Principles of Gunnery*, paſſes in this country for a ſtandard book, affirms, that gunpowder, when fired, generates a permanent elaſtic fluid of 250 times the bulk of the powder before it was fired. He found, that common air, which is heated by the contact of a red-hot iron, expands to four times its former bulk. Hence he concludes, that the elaſtic air, diſengaged from gunpowder, muſt expand alſo to four times its dimenſion; and that it occupies about a thouſand times the bulk of the powder in the moment of inflammation.

The learned Count ALGAROTTI remarks (b), that the elaſtic air generated by the inflammation of gunpowder occupies, when cold, 222 times the bulk of the powder, which agrees, as he finds, with the computation made by Meſſ. HAUKSBEE, AMONTONS, and BELIDOR. This calculation he confirms in the *Melanges de Philoſophie et des Mathematiques de la Societé Royale de* Turin, which is a continuation of the former work. He is of opinion, that this elaſtic fluid is of the ſame nature with common air (which was likewiſe the opinion of Dr. HALES); and that the prodigious force of gunpowder depends upon the action of the fire on all its parts, by which this fluid exerciſes all the force of its elaſticity.

The extrication of ſuch a conſiderable quantity of a permanent elaſtic fluid by the firing of gunpowder has been put to a particular uſe by ſeveral philoſophers.

The celebrated Mr. DE LA CONDAMINE gives an account of a braſs air gun contrived by one Mr. MATY, of Turin; which he loaded with air condenſed by firing in it two ounces of gunpowder, which, by its inflammation, let looſe ſuch a quantity of air as was ſufficient to ſhoot a leaden bullet ſixty paces, and to repeat the proceſs

(b) Miſcellanea Philoſophico-Mathematica Societatis privatæ Taurinenſis, p. 125.

eighteen

eighteen times, the ſtrength of the explosion diminiſhing gradually as in other air guns (d).

In the learned work of Mr. ANTONI, an Italian gentleman, I find the ſame experiment, the account of which is accompanied with a figure of ſuch an air gun, in which the author fired one ounce of gunpowder, the barrel being very ſtout, and of a ſize capable of containing ten ounces. He afterwards let a quantity of this compreſſed air out by a valve in the ſame way as it is done in the common air guns. This one ounce of gunpowder yielded air enough to propel a leaden bullet through a board three lines thick, at the diſtance of forty paces, and to repeat the proceſs ſixteen or eighteen times (e).

The difference of the quantity of elaſtic fluid obtained from the firing of gunpowder by Mr. ROBINS and others might be owing to the difficulties attending the inveſtigation, or to the different proportion of the ingredients uſed in the compoſition of the powder; as it is well known, that gunpowder for the uſe of the army is made of five or ſix parts of nitre to one of charcoal,

(d) Extrait d'un Journal de Voyage d'Italie, par M. DE LA CONDAMINE, inſéré dans les Memoires de l'Academie Royale des Sciences de Paris, 1757, p. 405.

(e) Examen de la Poudre traduit de l'Italien de M. ANTONI, par M. le Viſcomte de FLAVIGNY, 1773.

and

and one of ſulphur; when ſeven parts of nitre are uſed, it is called *poudre d'artifice*.

The celebrated Mr. JOHN BERNOUILLI calculates the denſity of the air contained in a ſolid ſtate in gunpowder to be $\frac{1}{1000}$ of what this fluid is when it conſtitutes a part of our atmoſphere. But he does not conſider this air as exiſting in all the component ingredients of the gunpowder, but chiefly in the nitre: and Count SALUCE ſuppoſes, that that part of the gunpowder which contains this air conſtitutes a conſiderable part of its bulk (though ſomewhat leſs than the half). Let us now ſuppoſe, that part of the gunpowder which contains this air to be not much leſs than the half of the whole maſs (for it would be difficult to demonſtrate accurately to what proportion of the whole maſs this part amounts in reality). On this ſuppoſition we ſhall find, that the whole maſs of gunpowder contains a quantity of air in a ſolid ſtate which is reduced in bulk to near $\frac{1}{500}$, or, in other words, that one ſquare inch of gunpowder contains near 500 ſquare inches of air; which being heated in the moment of inflammation will expand to four times its diameter; ſo that according to this calculation gunpowder muſt expand in the moment of exploſion to near 2000 times its own bulk.

It ſeems very probable, that this calculation of Mr. BERNOUILLI is much nearer the truth than that of

Mr. ROBINS and Count SALUCE; but yet the evaluation of the two laſt mentioned writers, though ſhort of one half, proves in reality Mr. BERNOUILLI's calculations to be as juſt as can be expected, when it is conſidered, that this evaluation was made before the new diſcoveries upon the nature of nitre and charcoal. But this aſſertion will be better underſtood when I have explained the nature of gunpowder ſomewhat fuller.

If we continue to ſay, as we have hitherto done, that the charcoal taking fire decompoſes the nitre, and extricates from it that amazing quantity of elaſtic fluid which was ſhut up within its ſubſtance, we only ſay what we ſee in reality is the conſequence of ſetting fire to this ingredient. But this explanation does not convey a clear idea of the manner in which the extrication is carried on; nor of the reaſon why one ſingle ſpark of fire, thrown into an immenſe heap of gunpowder, ſhould almoſt in an inſtant ſpread the conflagration through the whole maſs. Neither does it explain clearly, why nitre and charcoal (which ſeparately yield no flame at all, though ever ſo much heated) when combined and intimately mixed together, explode with as loud a report as a large ordnance piece, ſurpaſſing even in loudneſs thunder-claps; nor why this forcible exploſion is accompanied by a moſt brilliant flame.

Nitre

Nitre is compoſed of two different ingredients, *viz.* an acid, called from its peculiar nature the *nitrous acid*, and the vegetable alkali. Neither of theſe two ingredients are capable of inflammation; nay, they even extinguiſh actual fire. When they are both combined and conſtitute the neutral ſalt we ſpeak of, they have not, even by their coalition in one body, acquired an inflammable quality, for nitre may be made red-hot in a crucible without ſhewing the leaſt appearance of inflammation, not even when a red-hot ſtone or piece of iron is thrown into it. But if any common combuſtible ſubſtance, as wood, charcoal, or ſuch like, is thrown into the melted nitre, a flame iſſues with a kind of exploſion, though only at the very place where the two ſubſtances come into contact. The ſame flame and exploſion is obſerved when cold nitre is thrown upon a combuſtible body, in a ſtate of real ignition, on a piece of red-hot charcoal for inſtance.

The true reaſon of this wonderful phenomenon has not been conſidered hitherto with that degree of attention it deſerves, and could not have occurred to any body before our modern philoſophers had diſcovered the nature of various kinds of air, and the manner of extracting them from bodies.

Inflam-

Inflammable air, the *gas flammeum* of VAN HELMONT, was conſidered as an aerial fluid, ſuſceptible of inflammation by itſelf.

But we now know, that inflammable gas concentrated will not burn at all; but on the contrary extinguiſhes flame. Mr. CAVENDISH was the firſt who ſet this matter in a proper light. He diſcovered, by experiments, that a mixture of a ſmall quantity of common air with a great proportion of inflammable air, as, for inſtance, two parts of common air with eight parts of inflammable air, caught fire without noiſe, and conſumed gradually; but that three parts of inflammable air and ſeven parts of reſpirable air exploded with a very loud ſound.

It was Dr. PRIESTLEY's important diſcovery which ſuggeſted to me the theory I intend to lay before the Royal Society.

This acute philoſopher found, that, if inſtead of common air, dephlogiſticated air is mixed with a due proportion of inflammable air, the exploſion is conſiderably louder.

The principal ingredients of gunpowder, and thoſe to which it owes its force, are nitre and charcoal; for theſe two ingredients, well mixed together, conſtitute gunpowder at leaſt equal if not ſuperior in ſtrength to common gunpowder (as I found by experience, and may be

ſeen in the Memoire of Count SALUCE, inſerted in the *Melanges de Philoſophie et de Mathematiques de l'Acad. Royale de* Turin). The ſulphur ſeems to ſerve only for the purpoſe of ſetting fire to the maſs with a leſs degree of heat.

Nitre yields by heat a ſurprizing quantity of pure dephlogiſticated air. Charcoal yields by heat a conſiderable quantity of inflammable air. The fire employed to inflame the gunpowder extricates theſe two airs, and ſets fire to them at the ſame inſtant of their extrication. Thus the difference between the inflammation of gunpowder and that of a mixture of inflammable air with dephlogiſticated air in an ordinary air piſtol (as theſe laſt are now contrived for a philoſophical amuſement) ſeems to be, that the compound of the two airs in the air piſtol takes fire, when already extricated and exiſting in a ſpace without compreſſion or condenſation; that is to ſay, when they are in no condition of exerting a much greater elaſticity than what they acquired by the heat generated in the moment of their exploſion; which heat can only expand them to four times their former bulk, according to Mr. ROBINS: whereas in gunpowder the two airs, exiſting in a ſolid ſtate before their extrication, and occupying, according to Mr. ROBINS, about $\frac{1}{250}$ of the ſpace they take up after they are ſet looſe, but moſt probably

bably even leſs than $\frac{1}{500}$ (as will be ſeen by and by), are extricated all at once, when confined (as in fire arms) in a ſpace $\frac{1}{250}$ or rather $\frac{1}{500}$ times leſs than they can occupy when reduced to the temperature of the common atmoſphere, and of conſequence 2000 leſs than they can occupy when heated in the moment of inflammation; ſo that the difference of the exploſive force in the inflammation of the two compounds can be no leſs than as 4 is to 2000.

It muſt be here remembered, that air being a very compreſſible body, a moderate reſiſtance acting againſt its rarefaction eaſily overcomes the force of its expanſion, when this expanſion or rarefaction does not amount to more than four times its bulk (that ſuch a power ought not to be very great, we know by the force employed in ordinary wind guns and condenſing machines) whereas no condenſing machine has yet been contrived by which air could be condenſed to any thing approaching the ſtate of condenſation of this fluid as it exiſts in the ſubſtance of nitre.

It might be here objected, that air compreſſed to one tenth in a wind gun poſſeſſes a power not much ſhort of gunpowder, though only $\frac{1}{18}$ or $\frac{1}{20}$ of it is let looſe at a time; and that thus, inflammable air, though expanded only four times in the moment of inflammation, may exert a force approaching that of the wind gun, the whole

maſs of the charge being employed in one and the ſame exploſion. This compariſon is very inadequate; for in the caſe of a wind gun the air compreſſed to $\frac{1}{10}$ is ready to exert all the force of elaſticity exiſting in the whole maſs, and may therefore be compared to a ſtrong ſpring forcibly bent. But the inflammable air is far from exerting the force of expanſion and elaſticity through its whole maſs at the ſame inſtant: for the inflammation is propagated through it ſucceſſively, beginning where the electrical ſpark kindles it, and reaching gradually farther till the whole is conſumed. Now as I have demonſtrated, that inflammable air is reduced to more than half its bulk by inflammation, it muſt follow, that that portion of it which is conſumed the firſt by the inflammation, leaving more room by its diminution, diminiſhes in proportion the propelling powers of what remains ſtill to be inflamed.

The very great difference between the exploſive force of the two compounds is illuſtrated by what happens after their inflammation. The compound of inflammable with common or dephlogiſticated air, is very much reduced in bulk after inflammation. I found this by the following experiment: I fired a braſs inflammable air piſtol (made by Mr. NAIRNE according to my directions) which had a piſton in the cylinder, by which a proper

quantity

quantity of reſpirable and inflammable air was drawn in. I had rammed into the barrel, adapted to it, a leaden bullet wrapped up in a piece of leather ſo ſtrongly that I did not expect the reſiſtance could be overcome by the exploſion. I fired it by an electrical ſpark; the inflammation took place, the piſtol grew hot, the ball was not propelled, and the piſton was driven more than half way down the cylinder by the preſſure of the atmoſphere acting upon it when the exploſive air was conſumed by the inflammation.

The caſe is quite different in the firing of gunpowder, as there remains after its inflammation a maſs of air which occupies about 250 times the former bulk, according to Mr. ROBINS.

As, in the foregoing experiment, the compound of inflammable and common air was reduced above the half of its former bulk, it ſeems more than probable, that the quantity of dephlogiſticated and inflammable air extricated in the firing of gunpowder muſt alſo undergo a ſimilar diminution by its inflammation; ſo, that when there remains a maſs of air, 250 times the bulk of the gunpowder, the quantity of air extricated from the powder muſt have been in reality not leſs than 500 times the bulk of the powder, which agrees nearly with the calculation of Mr. JOHN BERNOUILLI. Let us now

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ſee how far this computation agrees with the analyſis of gunpowder. Abbé FONTANA, ſo advantageouſly known by his important diſcoveries in natural philoſophy, more eſpecially by thoſe he has made on the various kinds of air, favoured me with the following reſult of his experiments. An ounce of nitre, expoſed to a great degree of heat for the purpoſe of extracting its air in the uſual way, yielded about 800 cubic inches of dephlogiſticated air. An ounce of charcoal, treated in the ſame way, gave about 150 cubic inches of air, partly fixed, partly inflammable, mixed with ſome common air.

Let us now calculate (without, however, being too ſcrupulous about the accuracy of the reſult) what quantity of elaſtic permanent fluid a cubic inch of ſolid gunpowder will give in the moment of deflagration: a cubic inch of ſolid gunpowder contains in weight 442 grains (which is 38 grains ſhort of an ounce Troy weight) of which $331\frac{1}{2}$ grains is nitre, $55\frac{1}{4}$ charcoal, and as much ſulphur (ſuppoſing the proportion of the ingredients of the powder to be ſix parts of nitre to one of charcoal and one of ſulphur); $331\frac{1}{2}$ grains of nitre will give about 552 cubic inches of dephlogiſticated air; $55\frac{1}{4}$ grains of charcoal will produce about 17 cubic inches of air, chiefly inflammable, according to the calculation of Abbé FONTANA.

By

By this calculation, which will, perhaps, be found more accurate than the former, one cubic inch of ſolid gunpowder will yield above 569 cubic inches of permanent elaſtic fluid: I ſay, above 569 cubic inches, for I do not put into the account the elaſtic fluid which is generated by the ſulphur, nor that which charcoal, conſumed by the inflammation of the gunpowder, yields above the quantity mentioned, which it gives when heated in a glaſs veſſel, by which it is by no means conſumed, an ounce loſing by this operation only 60 grains of its weight.

As this elaſtic fluid will increaſe to four times its bulk, it follows, that one cubic inch of ſolid gunpowder will extricate in the moment of exploſion above 2276 cubic inches of elaſtic air. Which computation is not far from the reſult of my former calculation, and that of Mr. BERNOUILLI.

An accurate calculation of the expanſion of gunpowder would be a very difficult undertaking. The expanſion of the moiſture always contained in gunpowder, however dry, may alſo contribute its ſhare towards the amazing powers of this ingredient. Nitre contains from its nature a great ſhare of water, which is neceſſary for the cryſtallization of it, and charcoal is always found to contain it. We know,

that very hot vapour is capable of occupying almoſt 2000 times the ſpace it did in the ſtate of cold water.

The generation of dephlogiſticated and inflammable air by the inflammation of gunpowder is the reaſon why this ingredient is almoſt the only one known, which does not want a free acceſs of common air to be conſumed by fire; and therefore it may be ſaid to feed, as it were, upon its own air.

This theory of gunpowder induces me to venture a new one of the *pulvis fulminans*, which conſiſts of three parts of nitre, of two of fixed alkaline ſalt, and of one of ſulphur. This powder much ſurpaſſes the force of gunpowder in exploding, with a very loud report, in the open air when it is heated to a certain degree. It is commonly ſaid, that in the heating of this powder the ſulphur joins with the alkaline ſalt and conſtitutes an *hepar ſulphuris*, which riſing up in bubbles confines the air contained in them, which air at laſt becomes ſo powerfully expanded that it overcomes and breaks through the reſiſtance of the coercive bubbles of the *hepar ſulphuris*, with all the force of its elaſticity; which ſudden emerſion muſt naturally occaſion a proportional ſound. But I think, that the nitre contained in this powder, being heated, yields its dephlogiſticated air when the melting ſulphur yields inflammable air; at the ſame time the ſulphur conſtitutes

with

with the alkaline ſalt, an *hepar ſulphuris*, which riſing in rough bubbles confines this exploſive air generated. At length, however, the increaſing heat, which ſets fire to the ſulphur, ſets this exploſive air on fire alſo; which then following its own nature explodes with ſo much the more force from its having been entangled and confined within the bubbles of the *hepar ſulphuris*.

After what has already been ſaid, it will not be difficult to explain, why a ſingle ſpark of fire propagates the combuſtion with great rapidity through the whole maſs of gunpowder, however great. If we put a ſingle grain of gunpowder upon a red-hot iron, we ſee the particles of red-hot charcoal projected with great rapidity in every direction by the forcible exploſion of the two airs extricated in the manner before explained. Thus, if one or more grains, among a heap of others, are ſet fire to, the particles of red-hot charcoal being driven with great violence againſt the ſurrounding grains communicate their heat to all the particles of charcoal they hit, which particles, by heating the particles of nitre in cloſe contact with them, extricate their dephlogiſticated air at the ſame time that the charcoal yields its inflammable air; in conſequence of which a more powerful exploſion happens. This ſecondary exploſion projects with a much greater force the particles of charcoal ſurrounded by the exploſive

flame of the two airs; and thus the conflagration ſpreads with a very great velocity through the whole maſs, though always by ſucceſſion. The quickneſs of this propagation of fire depends in a great meaſure upon the intervals or interſtices which remain among the grains of gunpowder, through which the particles of heated charcoal are driven in every direction, together with the flame of the two airs. Hence gunpowder reduced into impalpable powder, and rammed into a ſquib, does not inflame with an exploſion, but burns ſlowly farther and farther till the combuſtion reaches the extremity of the ſquib, where it meets a maſs of gunpowder in grains, when immediately a loud exploſion iſſues, by which the ſquib is ſhattered into rags. Hence the ſize of the grains of gunpowder muſt be proportionate to the ſize of the fire arms to which it is deſtined, the greateſt fire arms requiring in general grains of the largeſt ſize.

If this wonderful and awful ingredient had not been diſcovered by accident, could the ſecret have eſcaped a long while the penetration of our modern philoſophers, who have found out the way of combining the air of the two conſtituents after they had extricated them, without any regard to the known properties of gunpowder? Nothing more was to be done than combining the two ſub-

ſtances, inſtead of combining the two airs firſt ſeparated from them.

APPENDIX.

IN the foregoing paper I attempted to give a comparative view of the exploſive force of gunpowder and inflammable exploſive air, which latter I had found to be ſo far ſhort of the exploſive force of gunpowder as not to conceive any well grounded hope that it could ever become a ſubſtitute to this ingredient.

At that time I had not yet tried the effect of very pure dephlogiſticated air combined with that inflammable air, into which I had found that vitriolic æther is changed in an inſtant.

I muſt acknowledge, that I had but ſmall expectations from the force of theſe two airs combined; for as I had always obſerved, that æther air combined with common air is leſs briſk in taking fire, and leſs powerful in exploding, than inflammable air extracted from the vitriolic or marine acid, I thought that the ſame æther air combined with very pure dephlogiſticated air would alſo

be leſs powerful than common inflammable air from metals. But how far experience contradicted this theoretical analogy will be ſeen in the following lines.

Abbé FONTANA was ſo good as to aſſiſt me in this purſuit. Having produced a good quantity of pure dephlogiſticated air from red precipitate by heat, we firſt filled a ſtrong two-ounce phial (the orifice of which was ſo wide that it could ſcarce be covered with the thumb, ſo that the bottle was almoſt cylindrical) with this air, in the uſual manner, by filling it firſt with water, inverting it, and letting the air riſe in it; which being done, we dropped one drop of æther (in which a ſmall quantity of camphire was diſſolved) into it, and ſhut it immediately with the thumb. After having given it ſome concuſſions, the orifice was applied to the flame of a candle, by withdrawing the thumb when the orifice was cloſe to the flame: the air inſtantly took fire, and exploded with ſuch a ſtrong report, that, if the phial had not been very ſtout, it would moſt probably have been ſhattered into pieces, notwithſtanding its wide orifice. We repeated the ſame experiment with the ſame ſucceſs.

I was the more aſtoniſhed at the uncommcn loud report (conſidering the wide orifice of the phial), becauſe, having often tried æther air in the ſame way with common air, I never found it explode with any con-

conſiderable degree of force; and therefore I found it neceſſary, in order to procure a loud report, to kindle it by an electrical ſpark directed through the piſtol, when its orifice was ſhut up by a cork, the reſiſtance of which was the chief cauſe of the report.

This wonderful effect in an open veſſel could not fail of giving me a good expectation of a very powerful effect, if this compound air was ſhut up in an air piſtol by a cork ſqueezed into its orifice. As it had been now kindled twice by the flame of a candle, I wanted to kindle it by the ſame means in an air piſtol; for this purpoſe we drilled a ſmall hole in the ſide of the piſtol, which was made of tin, and contained about nine cubic inches of ſpace. We filled it with dephlogiſticated air in the ſame manner as we had filled the phial by means of water; and after having poured into it one drop of æther by means of a glaſs tube (in the manner above deſcribed), we ſhut the orifice by thruſting a cork into it, and kept a finger applied to the touch-hole which was drilled in the ſide of the piſtol. To avoid accidents if the piſtol ſhould burſt, we thought it prudent to ſqueeze the cork very gently into the orifice, ſo that the reſiſtance ſhould be very moderate. Abbé FONTANA wrapped a towel round the piſtol for ſecurity's ſake, leaving only the touch-hole uncovered; which being brought near the flame

of

of a wax taper, the air inſtantly took fire, and exploded with ſuch a ſtrong report, that his hearing, as well as mine, was much hurt by it. The cork, which was a very ſound one, flew to pieces againſt the wall; and the Abbé felt ſuch a conſiderable ſhock in his hands, that he did not think it ſafe to repeat the experiment, unleſs a ſtronger piſtol could be procured.

Encouraged by ſuch uncommon and unexpected effects, I went immediately to Mr. NAIRNE to inquire, whether he ſtill had in his poſſeſſion a ſtrong braſs air piſtol, which he had made laſt ſummer according to my direction? I was lucky enough to find it: nothing was to be done to it but to drill a touch-hole in the left ſide of it, in order to kindle it by a flame if required. This touch-hole was to be ſhut up by a braſs male ſcrew fitted exactly to it, when the piſtol was intended to be fired by an electrical ſpark.

The air box of this piſtol was a cylinder four inches long and two inches in diameter. The fore part of the air box to which the piſtol barrel fitted to receive a leaden ball or a cork, was fixed, had a broad ſhoulder, which was faſtened to the body of the air box by ſix ſtrong braſs ſcrews, which never had been looſened by former exploſions. A leaden bullet, wrapped up in leather, was forcibly rammed into the piſtol barrel as far as the ſcrew, which

joins

joins the barrel with the air box (as may be ſeen in the figure). The piſtol was filled with pure dephlogiſticated air (which was drawn-in by the piſton from an elaſtic gum bottle), and one drop of æther being poured into it, the air within was kindled by an electrical ſpark directed through it. The air took fire: the exploſion was as loud as that of a common muſket, and the force ſo great, that the whole fore part of the air box with the piſtol barrel flew off, all the ſix ſcrews were broke, and the ſtrong and tough metal of which they were made was rent. Three ſtrong braſs ſcrews, by which the bottom of the air box was fixed to the wooden handle, were looſened, and the whole frame of the piſtol was out of order. The ſubſtance of the air barrel, where it was tore, was of the thickneſs of about a half crown piece.

Being now convinced, that though inflammable air from metals with dephlogiſticated or common air, is far inferior to the force of gunpowder, the exploſive force of the compound of dephlogiſticated and æther air approaches it much nearer, I thought it worth while to fit the piſtol up in ſuch a manner as to be out of all danger of burſting. For this purpoſe I deſired Mr. NAIRNE to adapt, and ſolder to the fore part of the air box, a hollow cone of braſs, the extremity of which ſhould terminate in the gun barrel.

As

As the piſton could not reach to the extremity of this conical hollow (which conſequently muſt be always filled with common air), I deſired him to fix to the piſton an ivory cone, through which the two wires would paſs to meet one another at the ſurface of the cone, leaving an interſtice between them of about one line, through which the electrical ſpark ſhould leap and ſet fire to the air. This ivory cone ſhutting up exactly the whole cavity of the air box, no air could come into it but what was drawn in by the piſton.

The piſtol thus fitted up anſwered tolerably well. The figure joined to it will ſerve to give a better idea of the whole contrivance than could be well explained by words. The ſcale is in the proportion of one third of the real ſize.

The cone, inſtead of ivory, may be made of ſolid glaſs, which is a better non-conductor than ivory. The canals in the ivory, through which the two wires paſs, may be made wide enough to contain a glaſs tube, through which the wires paſs; or to be filled with a non-conducting cement, as ſealing wax, for the ſame purpoſe. The cone may even be made of braſs, provided two glaſs tubes are lodged in it, to give a paſſage to the two wires.

I kindle this piſtol ſometimes by putting in the touch-hole a little bit of a cotton thread ſoaked in moiſt gun-

powder

powder and dried afterwards; or a bit of thoſe paper matches which the Chineſe put into thoſe little ſquibs, which go by the name of *India crackers*. I ſometimes kindle it by holding the flame of a candle or a burning paper to the touch-hole. In this caſe it is to be obſerved, that the touch-hole muſt be kept upwards, if the piſtol is loaded with inflammable air from metals, becauſe this air being lighter than common air, will riſe out of the hole and meet the flame. The contrary muſt be done when æther air is employed, it being heavier than common air, and thus diſpoſed to deſcend and fall upon the flame kept under it.

To fill this piſtol with any air, I commonly firſt fill an elaſtic gum bottle with it, the orifice of which is juſt big enough to receive that part of the gun barrel which is fixed to the air box: thus, by ſqueezing between my feet the elaſtic gum bottle, I draw in at the ſame time the air by drawing up the piſton. A bladder is alſo very fit for this purpoſe, and has the advantage above an elaſtic gum bottle in not requiring to be ſqueezed to draw the air out of it.

Inflammable air from metals will riſe in the piſtol of itſelf, when its orifice is kept upon the bottle containing it.

If the piſtol is deſtined to be always kindled by the flame of a candle or a match, as I have deſcribed, it would be better to have no piſton to it, as it may then be filled by the means of water, and the exploſive force will be ſo much the greater, as ſome of the flame makes eaſily its way over the leather of the piſton, and ruſhes out backward, which, I find, is often the caſe, if the bullet is rammed in the barrel ſomewhat too tightly.

It would, perhaps, not be an eaſy undertaking to give a ſatisfactory reaſon, why a drop of æther communicates to dephlogiſticated air a much ſtronger exploſive force than common inflammable air from metals. May it not be ſaid, that common inflammable air from metals, having only about one fifth of the ſpecific gravity of the dephlogiſticated air, the two fluids do not penetrate one another ſo readily and ſo intimately as the compound of dephlogiſticated and æther air, which are both nearly of the ſame ſpecific gravity, each being ſomewhat heavier than common air? for it ſeems not improbable, that the ſwiftneſs with which the flame is propagated through the maſs of this compound air, depends partly on the intimate mixture of the phlogiſton with the dephlogiſticated air. Might not this phenomenon be aſcribed to the greater bulk of inflammable air from metals compared with the ſmall compaſs which one ſingle

drop

drop of æther occupies, which laſt ingredient, when pure, ſeems to be an eſſence of the inflammable principle of the ſpirit of wine, a pure phlogiſton concentrated in the form of a liquid? Indeed the inflammable air from metals ſeems to be rather a compound of phlogiſton and ſome kind of elaſtic permanent fluid than a pure inflammable fluid; for this air, after having loſt all its inflammability, by being kept a long while upon water, occupies ſtill a conſiderable ſpace, and is then become phlogiſticated air; that is to ſay, ſuch an air as is not to be diminiſhed by nitrous air, or to be inflamed.

Though I have no reaſon to alter my former aſſertion, that the force of gunpowder is proportionable to the ſudden extrication of a great quantity of the elaſtic fluid generated in the moment of conflagration, and the expanſion of this fluid by heat, communicated to it in the ſame moment of its extrication; and that the force of inflammable exploſive air can only be proportionable to the ſudden expanſion by heat in the moment of the inflammation (for no new extrication here takes place); yet I did not conſider enough in the account the ſuddenneſs of this expanſion, which may make a conſiderable difference in the force of the exploſion. And indeed the abovementioned experiments ſeem to demonſtrate, that the inflammation of the compound of pure dephlo-

gisticated and æther air spreads with such a velocity through the whole mass as to be almost instantaneous.

It is well known, that mechanical power chiefly depends upon the velocity with which a body is endowed in the instant of exerting it; or that the *momentum*, or force of a body, must be computed by multiplying the quantity of matter into the velocity with which it moves. Thus, if this new compound of dephlogisticated and æther air expands with ten times greater velocity than any other inflammable explosive air, its force will be about ten times greater.

As it seems to be probable, from what is already said that this compound of explosive air may be put to more uses than that of an amusing experiment, I think it worth while for men engaged in this branch of natural philosophy to look out for a method of producing at pleasure any quantity of dephlogisticated air required. Considering the rapid progress which is daily made on the important subject of air, I cannot but flatter myself, that this great discovery is not far off. The benefit which would arise from such a discovery for animal life must encourage every philosopher to pursue this object. Indeed, if we consider that nitre contains this wonderful aerial fluid in a most concentrated state, and that the nitrous acid seems to be nothing else but this beneficial

fluid

fluid combined with phlogiſton, which ſeems to be imbibed by the vegetable alkali, when the acid is expelled by heat in the form of this air; that this beneficial aerial fluid exiſts alſo, in a moſt concentrated ſtate, in bodies almoſt every where to be found, as are calces of metal, principally that of iron; that common water contains it in great abundance, ſo that the light and warmth of the ſun extracts it to one fifteenth of the bulk of the water, as Dr. PRIESTLEY found, that even the maſs of our atmoſphere is nothing elſe but this very air ſoiled with impurities. If we conſider, I ſay, all this, is it not reaſonable to hope, that we are near the important inſtant when this ſalubrious aerial fluid will be procured for many uſeful purpoſes in a ſufficient quantity, either by the diſcovery of a ready way to let looſe this air from the bodies in which it is as it were impriſoned, or by filtrating or purifying common air from its impurities?

EXPLANATION OF THE FIGURE.

A *a* the piſtol barrel.

B the large cylinder or air box.

b the place where the piſtol barrel unſcrews from the air box.

C the piſtol handle.

D the

D the handle fixed to the piſton at *h h*, which is ſquare to prevent the piſtons turning round.

E the hole in the ſide of the air box.

e the ſcrew to ſtop the ſaid hole.

F a piece of braſs with a female ſcrew, which was fixed with three ſtrong ſcrews to the wooden handle.

f a ſcrew at the end of the air box which ſcrewed into the piece F.

G a piece of ivory fixed to the piſton.

K the piſton, with the piece of conical ivory fixed to it.

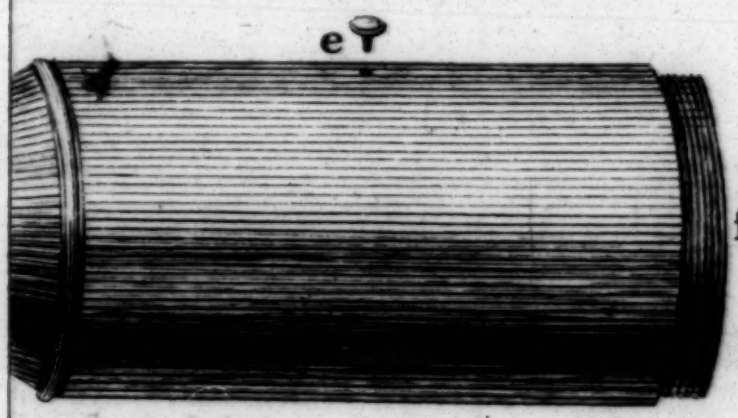
L the termination of the ivory cone which filled up the ſmall end of the air box.

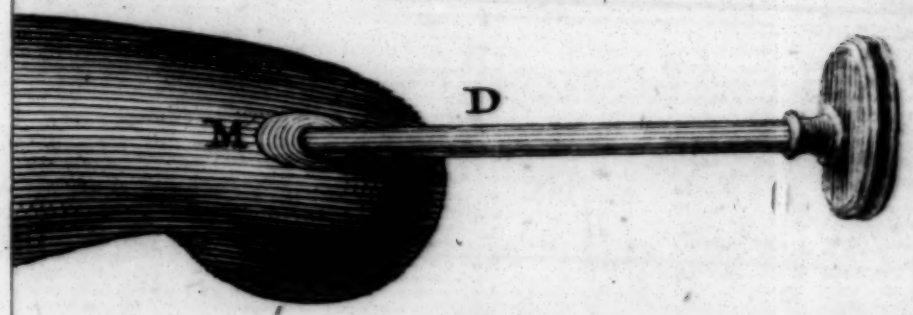
M the perforation in the wooden handle, through which the braſs handle D paſſes.

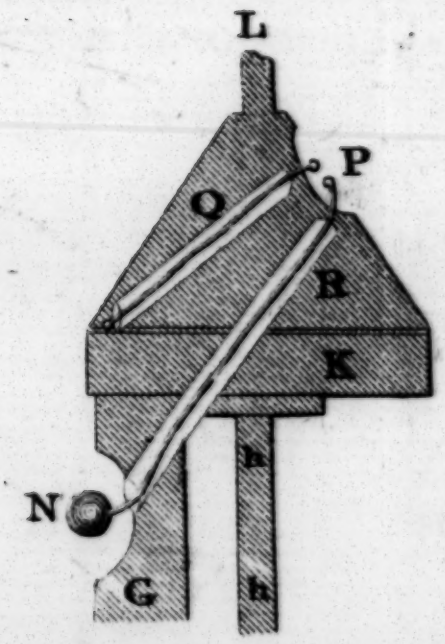
N a braſs ball at the end of a wire, which paſſes down the canal R in the ivory cone.

O another piece of wire paſſing down the other canal, canals are filled with non-conducting ſubſtance.

P an interſtice between the two wires of about one line, through which the electrical ſpark, when given on the ball N, paſſes and ſets fire to the inflammable air in the air box.







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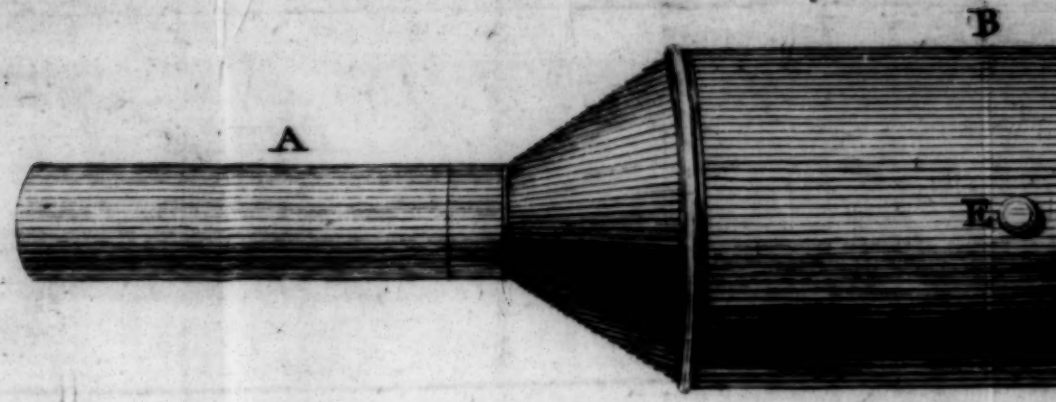
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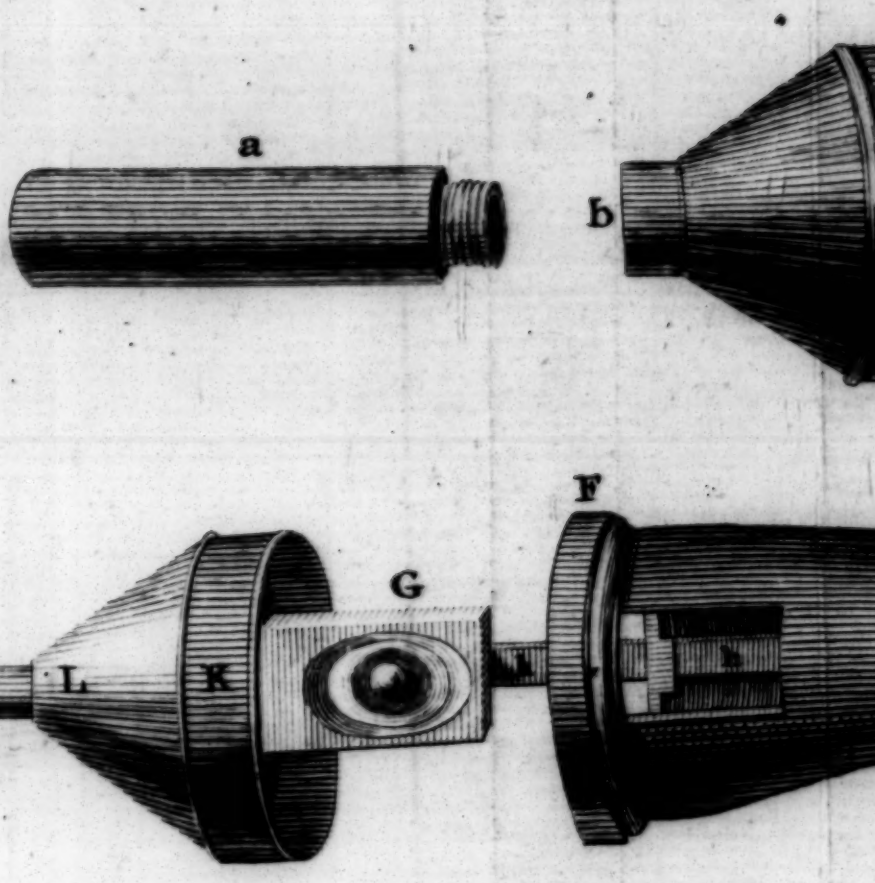
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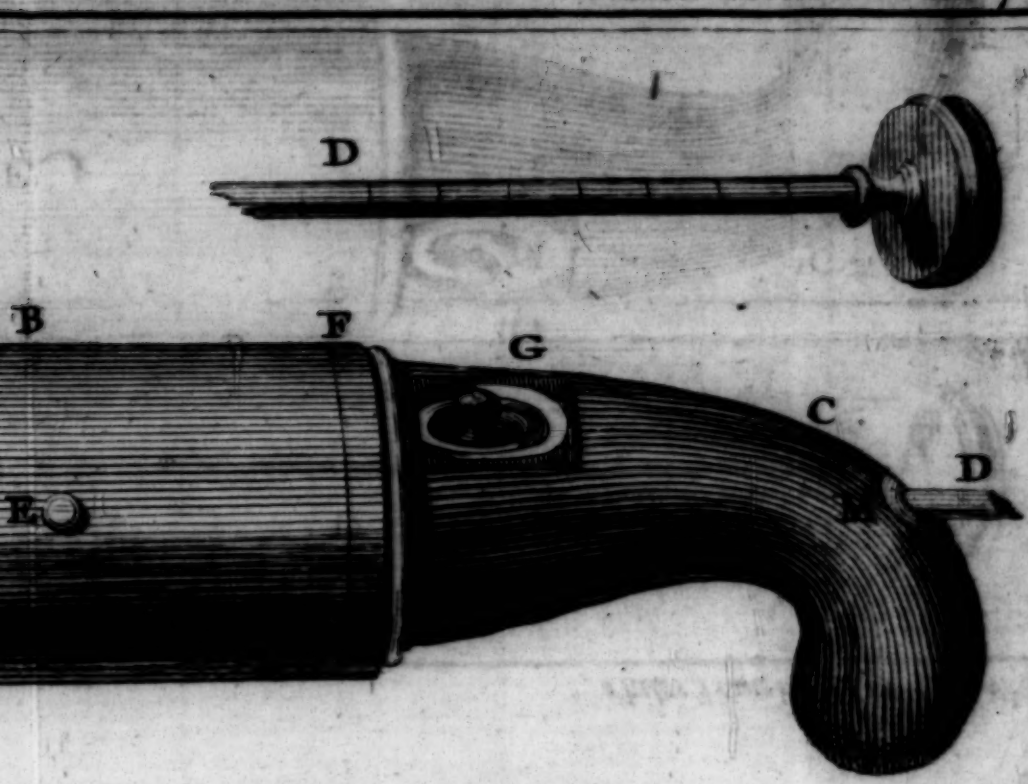
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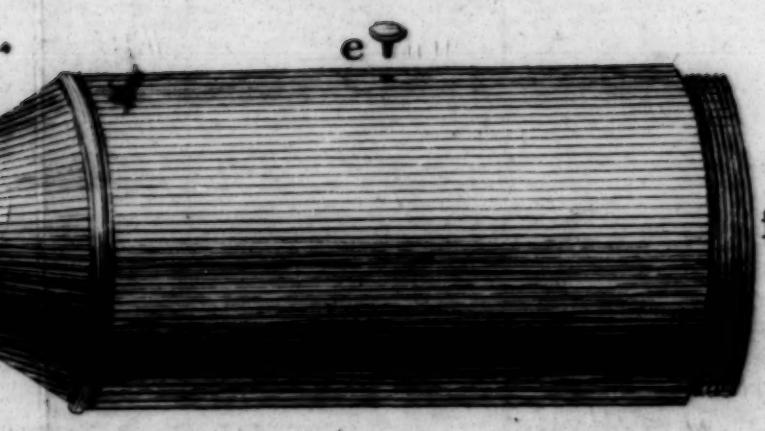
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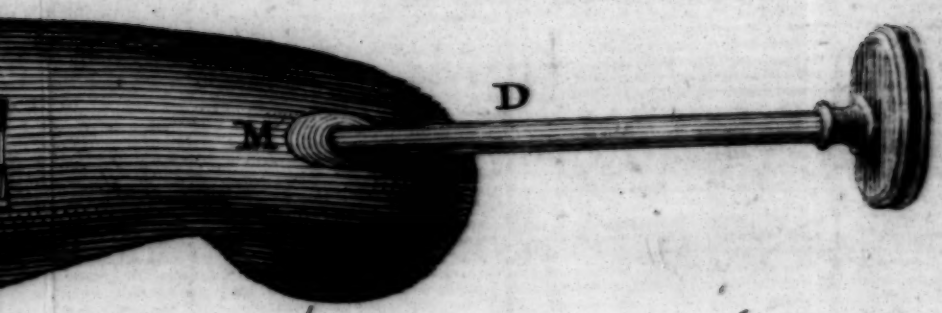
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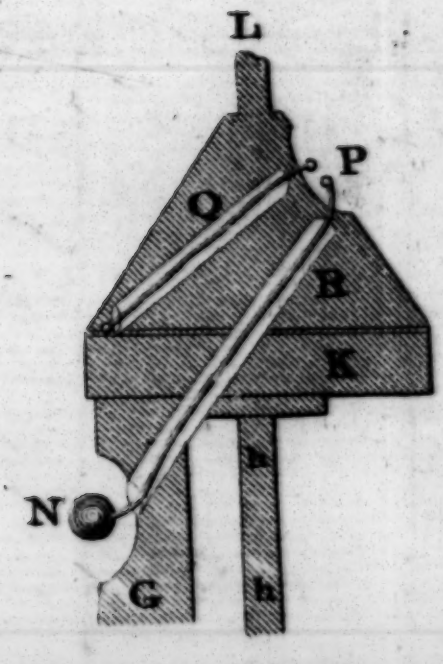
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XVII. *The Deſcription of Two new Micrometers.* *By Mr.* Ramſden, *Optician*; *communicated by* Joſeph Banks, *Eſq. P. R. S.*

Read March 25, 1779.

WHEN I offer the deſcription of a new micrometer to the learned members of the Royal Society, I do not flatter myſelf that mere novelty will entitle it to their patronage. Senſible how much the theory of aſtronomy is limited by the imperfection of inſtruments, I always incline to improve rather than invent, except when repeated examinations convince me, that the imperfections ariſe from defect in principle as well as in the conſtruction.

In conſidering micrometers, when an obſerver finds, that with the micrometer, which depends on moveable parallel wires, he cannot meaſure any diameter of a planet, except that which is at right angles to the direction of its apparent motion, he cannot withhold his preference to that conſtruction which meaſures the angle by the ſeparation of the images. It appeared therefore

to

to me a matter of ſome importance, to inveſtigate the cauſes of the uncertainty which has been found in the obſervations made with the micrometer with a divided object-glaſs. The reſult of my examination convinced me, that were it poſſible to execute the conſtruction of that micrometer, with the degree of accuracy required, it muſt ſtill be ſubject to inaccuracy from its principle.

By the poſition of the micrometer every error of its glaſs is magnified by the teleſcope; and if each ſurface of the micrometer glaſs has not, in every part, preciſely the ſame radius (which opticians muſt allow to be exceedingly difficult) there will be a conſiderable error in the angle to be meaſured, and the eye applied to the different parts of the pencil will, without moving the micrometer, ſee the images of the object in the teleſcope fluctuating, ſometimes appearing to overlap, and ſometimes to ſeparate from each other.

But ſuppoſing the glaſs itſelf to be perfect in its ſubſtance and in its curvature, there will yet remain imperfections which ariſe from its principle. A micrometer glaſs applied to a teleſcope cauſes a very conſiderable abberration. If the focus of the glaſs is poſitive, the extreme aberration will be within the geometrical focus; if negative, it will be beyond it: and the aberration not only affects

affects the distinctness of the image, but also the angle measured by the micrometer.

At the time I took up this subject, the divided object-glass micrometer was the only one which measured angles by the separation of two images. Since that time, a very ingenious application of the prism to this purpose has been invented by the rev. Dr. MASKELYNE, Astronomer Royal; and although experience has not yet ascertained the extent of its merit, it will always deserve great consideration from its ingenuity; but the more I considered the subject, I became more fully convinced, that the principle of reflection applied to micrometers would have great advantages over those hitherto constructed on the principle of refraction; and the catoptric micrometer I have the honour to describe, besides the advantage it derives from the principle of reflection, of not being disturbed by the heterogeniety of light, avoids every defect of other micrometers, and can have no aberration, nor any defect which arises from the imperfection of materials, or of execution, as the extreme simplicity of its construction requires no additional mirrors or glasses to those required for the telescope: and the separation of the image being effected by the inclination of the two specula, and not depending on the focus of any lens or mir-

ror, any alteration in the eye of an obſerver cannot affect the angle meaſured.

It has, peculiar to itſelf, the advantages of an adjuſtment to make the images coincide in a direction perpendicular to that of their motion; and alſo of meaſuring the diameter of a planet on both ſides the zero, which will appear no inconſiderable advantage to obſervers who know how much eaſier it is to aſcertain the contact of the external edges of two images than their perfect coincidence. A ſhort explanation of the annexed drawings will make the conſtruction and the properties of this micrometer obvious.

I divided the ſmall ſpeculum of a reflecting teleſcope, of CASSEGRAIN's conſtruction, into two equal parts, by a plane acroſs its center; and by inclining the halves of the ſpeculum to each other on an axis at right angles to the plane that ſeparated them, I obtained two diſtinct images. The ſatisfaction I received on the firſt trial was checked by the apparent impoſſibility of reducing this principle to practice. The angular ſeparation of the two images in this caſe being half the angular inclination of the two ſpecula, it required an index of an unmanageable length, to allow the quantity of one ſecond of a degree to become viſible. Some time afterwards, on reviſing the principle, I conſidered, that if both the halves of the mir-

for turned on their center of curvature, there could be no alteration in their relative inclination to each other from their motion on this center; and that any extent of scale might be obtained, by fixing the center of motion at a proportional distance from the common center of curvature. This will be better understood from the annexed drawing.

A (fig. 1.) represents the small speculum divided into two equal parts; one of which is fixed on the end of the arm B; the other end of the arm is fixed on a steel axis X, which crosses the end of the telescope C. The other half of the mirror A is fixed on the arm D, which arm at the other end terminates in a socket *y*, that turns on the axis, X; both arms are prevented bending by the braces *a a*. G represents a double screw, having one part *e* cut into double the number of threads in an inch to that of the part *g*: the part *e* having 100 threads in one inch, and the part *g* 50 only. The screw *e* works in a nut F in the side of the telescope, while the part *g* turns in a nut H, which is attached to the arm B; the ends of the arms B and D, to which the mirrors are fixed, are separated from each other by the point of the double screw pressing against the stud *b*, fixed to the arm D, and turning in the nut H on the arm B. The two arms B and D are pressed against the direction of the double screw *eg* by

a ſpiral ſpring within the part n, by which means all ſhake or play in the nut H, on which the meaſure depends, is intirely prevented.

From the difference of the threads on the ſcrew at *e* and *g* it is evident, that the progreſſive motion of the ſcrew through the nut will be half the diſtance of the ſeparation of the two halves of the mirror, and conſequently the half mirrors will be moved equally in contrary directions from the axis of the teleſcope C.

The wheel V fixed on the end of the double ſcrew has its circumference divided into 100 equal parts, and numbered at every fifth diviſion with 5, 10, &c. to 100, and the index I ſhews the motion of the ſcrew with the wheel round its axis, while the number of revolutions of the ſcrew is ſhewn by the diviſions on the ſame index. The ſteel ſcrew R may be turned by the key S, and ſerves to incline the ſmall mirror at right angles to the direction of its motion. By turning the finger head T (fig. 2.) the eye tube P is brought nearer or farther from the ſmall mirror, to adjuſt the teleſcope to diſtinct viſion; and the teleſcope itſelf hath a motion round its axis for the conveniency of meaſuring the diameter of a planet in any direction. The inclination of the diameter meaſured with the horizon is ſhewn in degrees and minutes by a level and vernier on a graduated circle, at the breech of the teleſcope.

The

The method of adjuſting and uſing the catoptric micrometer is too obvious to require any explanation: it is only neceſſary to obſerve that, beſides the table for reducing the revolutions and parts of the ſcrew to minutes, ſeconds, &c. it may require a table for correcting a very ſmall error which ariſes from the excentric motion of the half mirrors. By this motion their centers of curvature will (when the angle to be meaſured is large) approach a little towards the large mirror; the equation for this purpoſe in ſmall angles is inſenſible, but when angles to be meaſured exceed ten minutes, it ſhould not be neglected. Or, the angle meaſured may be corrected by diminiſhing it in the proportion the verſed ſine of the angle meaſured, ſuppoſing the excentricity radius, bears to the focal length of the ſmall mirror.

The teleſcope to which the catoptric micrometer is applied is of the CASSEGRAIN conſtruction. The great ſpeculum is about twenty-two inches focus, and bears an aperture of $5\frac{5}{10}$ inches, which is conderably larger than thoſe of the ſame focal length are generally made: indeed, the apparent utility of this micrometer makes me wiſh to ſee the reflecting teleſcope meet with further improvements. I believe it would more tend to the advancement of the art of working mirrors, if writers on this ſubject, inſtead of giving us their methods of working

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imaginary parabolas, would demonſtrate the properties of curves for mirrors which, placed in a teleſcope, will ſhew images of objects perfectly free from aberration; or, what will yet be more uſeful in practice, of what forms ſpecula might be made, that the aberration cauſed by one mirror may be corrected by that of the other. If mathematicians aſſume *data* which really exiſt, they muſt ſee, that when the two ſpecula of a reflecting teleſcope are parabolas, they cauſe a very conſiderable aberration, which is negative, that is to ſay, the focus of the extreme rays is longer than thoſe of the middle ones. If the large ſpeculum is a parabola, the ſmall one ought to be an ellipſe; but when the ſmall ſpeculum is ſpherical, which is generally the caſe in practice, if concave, the figure of the large ſpeculum ought to be an hyperbola; if convex, the large ſpeculum ought to be an ellipſe, to free the teleſcope from aberration.

This will be eaſier underſtood by attending to the poſitions of the firſt and ſecond images; when a curve is of ſuch form that lines drawn from each image, and meeting in any part of the curve, make equal angles with the tangent to the curve at that point, it is evident, that ſuch curve will be free from aberration.

This is the property of a circle when the radiant and image are in the ſame place; but when they recede

from

from each other, of an ellipſe of ſuch form that the radiant and image are in the two *foci*, till one diſtance becoming infinite the ellipſe changes into a parabola, and to an hyperbola when the focus is negative; that is to ſay, when reflected rays diverge, and the focus is on the oppoſite ſide of the mirror.

Theſe principles made me prefer CASSEGRAIN's conſtruction of the reflecting teleſcope to either the Gregorian or Newtonian. In the former, errors cauſed by one ſpeculum are diminiſhed by thoſe in the other.

From a property of the reflecting teleſcope (which has not been attended to) that the apertures of the two ſpecula are to each other very nearly in the proportion of their focal lengths, it follows, that their aberrations will be to each other in the ſame proportion, and theſe aberrations are in the ſame direction, if the two ſpecula are both concave; or in contrary directions, if one ſpeculum is concave, and the other convex.

In the Gregorian conſtruction, both ſpecula being concave, the aberration at the ſecond image will be the ſum of the aberrations of the two mirrors; but in the CASSEGRAIN conſtruction, one mirror being concave, and the other convex, the aberration at the ſecond image will be the difference between their aberrations. By aſſuming ſuch proportions for the *foci* of the ſpecula as are gene-

rally uſed in the reflecting teleſcope, which is about as 1 to 4, the aberration in the CASSEGRAIN conſtruction will be to that in the GREGORIAN as 3 to 5.

I have mentioned theſe circumſtances in hopes of recommending the demonſtration of curves ſuited to the purpoſes of optics to the attention of mathematicians, which would be of great uſe to artiſts.

I ſhall conclude this paper with the deſcription of a new micrometer ſuited to the principle of refraction; being ſenſible that both principles have their peculiar advantages. Though the former part of this paper proves my partiality to the principle of reflection applied to micrometers, yet the very favourable opinion I have of the refracting teleſcope made me attentively conſider ſome means of applying a micrometer to it, which might obviate the errors complained of in the former part of this paper.

The application of any lens or medium between the object glaſs and its focus muſt inevitably deſtroy the diſtinctneſs of the image; I therefore have employed for the micrometer glaſs one of the eye glaſſes requiſite in the common conſtruction of the teleſcope; but if it ſhould be found neceſſary to apply an additional eye glaſs for the conveniency of enlarging the ſcale, I am able thereby to cor-

out both the colours and ſpherical aberration of the firſt eye glaſs.

This micrometer is applied to the erect eye tube of a refracting teleſcope, and is placed in the conjugate focus of the firſt eye glaſs: hence ariſes its great ſuperiority to the object glaſs micrometer. It has been before obſerved, that if a micrometer is applied at the object glaſs, the imperfections of its glaſs are magnified by the whole power of the teleſcope; but in *this* poſition, the image being conſiderably magnified before it comes to the micrometer, any imperfection in its glaſs will be magnified only by the remaining eye glaſſes, which in any teleſcope ſeldom exceeds five or ſix times.

By this poſition the ſize of the micrometer glaſs will not be the $\frac{1}{100}$th part of the area which would be required if it was placed at the object glaſs; and, notwithſtanding this great diſproportion of ſize, which is of great moment to the practical optician, the ſame extent of ſcale is preſerved, and the images are uniformly bright in every part of the field of the teleſcope.

Fig. 4th, repreſents the glaſſes of a refracting teleſcope; *xy* the principal pencil of rays from the object glaſs o; *tt* and *uu* the axis of two oblique pencils; *a* the firſt eye glaſs; *m* its conjugate focus, or the place of the micrometer; *b* the ſecond eye glaſs, *c* the third, and *d* the

fourth, or that which is nearest the eye. Let p be the diameter of the object glaſs, e the diameter of a pencil at m, and f the diameter of the pencil at the eye; it is evident, that the axis of the pencils from every part of the image will croſs each other at the point m, and e, the width of the micrometer glaſs, is to p the diameter of the object glaſs as ma is to go, which is the proportion of the magnifying power at the point m, and the error cauſed by an imperfection in the micrometer glaſs placed at m will be to the error, had the micrometer been at o, as m is to p.

Fig. 3d, repreſents the micrometer; A a convex or concave lens divided into two equal parts by a plane acroſs its center; one of theſe ſemi-lenſes is fixed in a frame B, and the other in the frame E, which two frames ſlide on a plate H, and are preſſed againſt it by thin plates *aa*: the frames B and E are moved in contrary directions by turning the button D; L is a ſcale of equal parts on the frame B; it is numbered from each end towards the middle with 10, 20, &c. There are two verniers on the frame E, one at M, and the other at N, for the conveniency of meaſuring the diameter of a planet, &c. on both ſides the zero. The firſt diviſion on both theſe verniers coincides at the ſame time with the two zero's on the ſcale L, and,

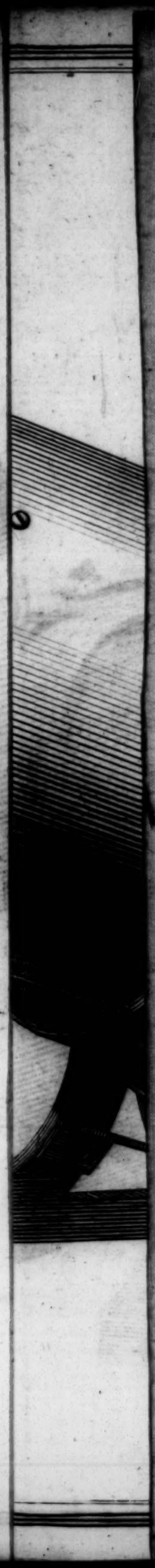
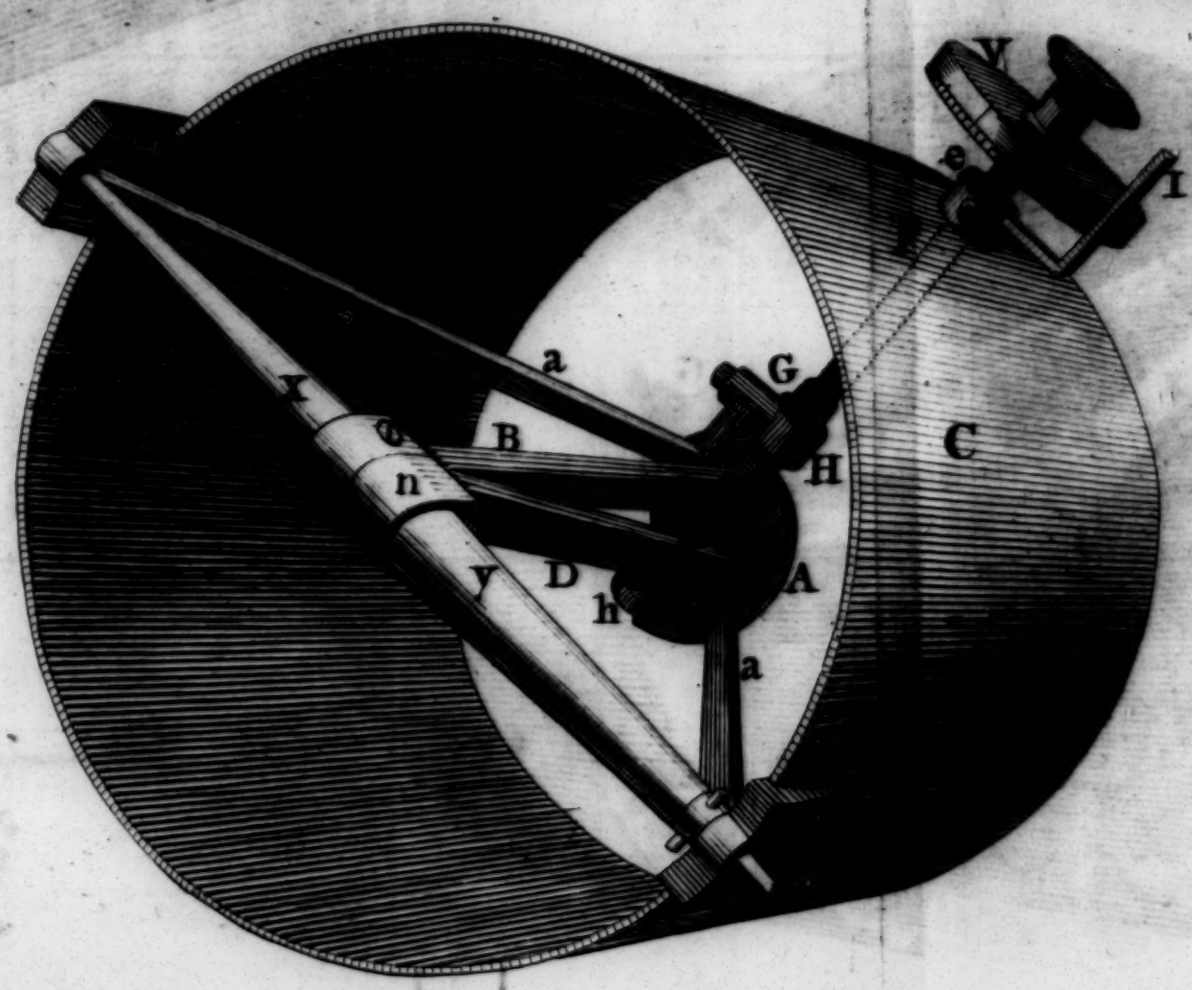


Fig: 1.



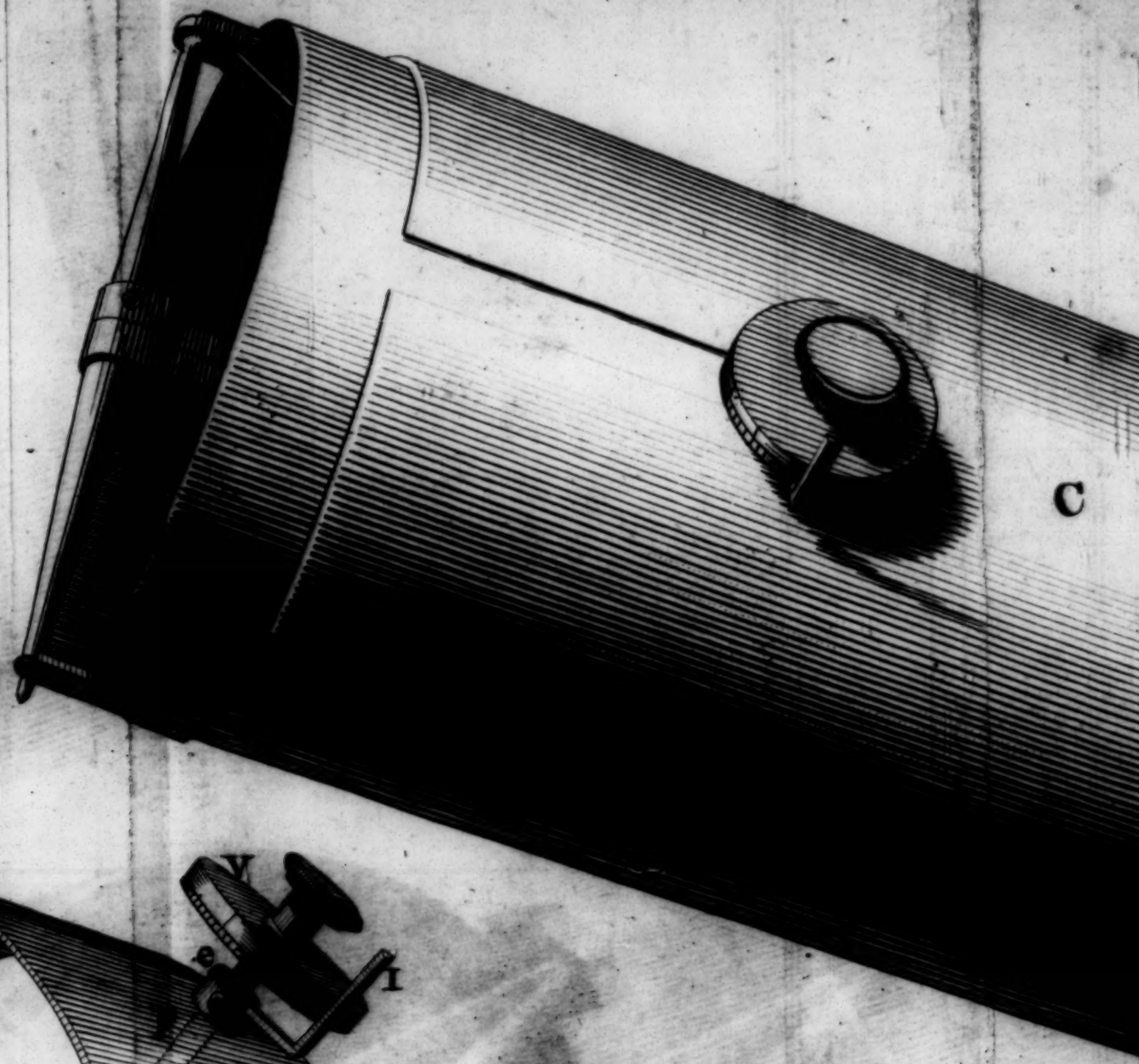


Fig: 2.

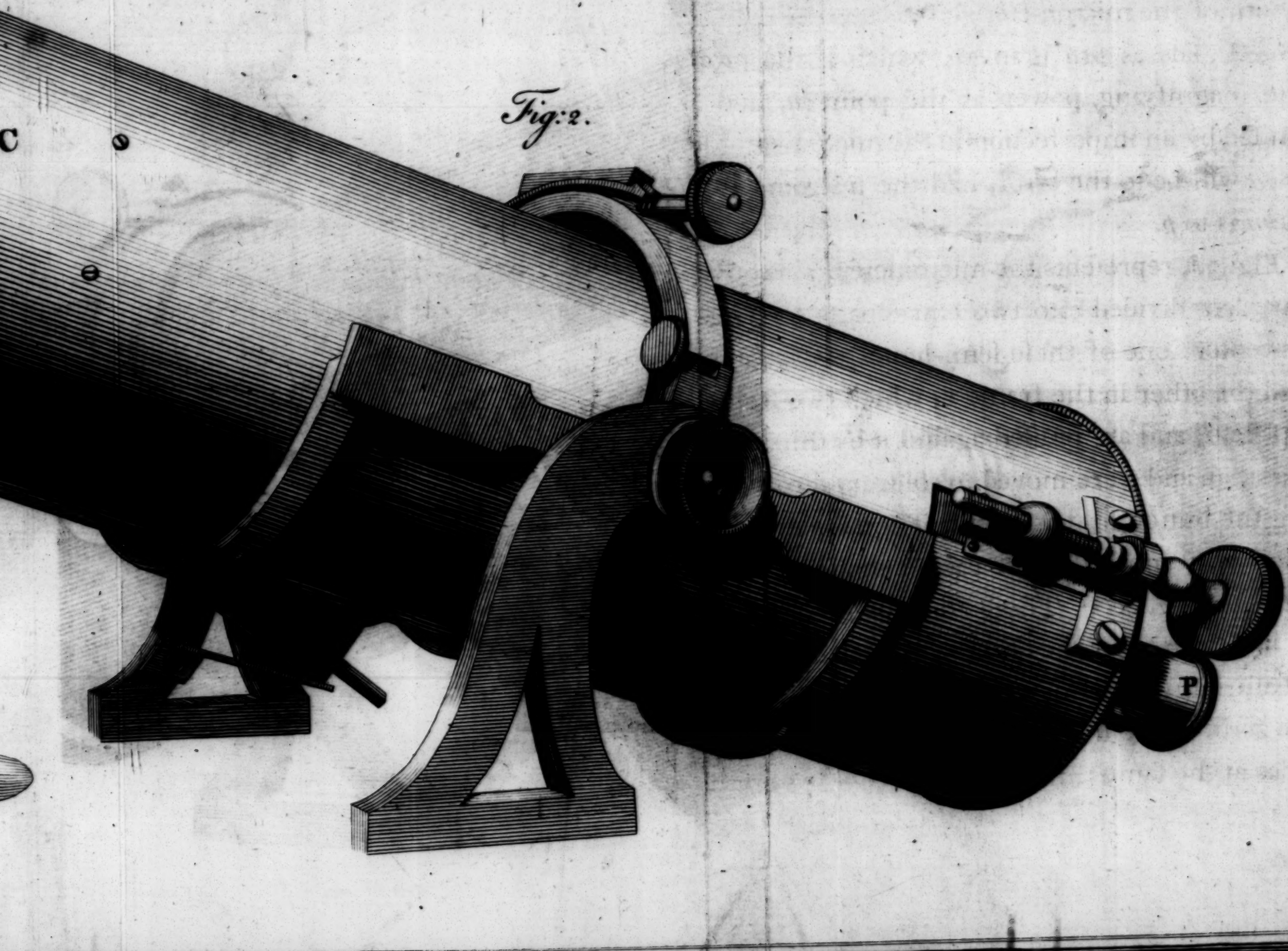


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Fig: 3.d

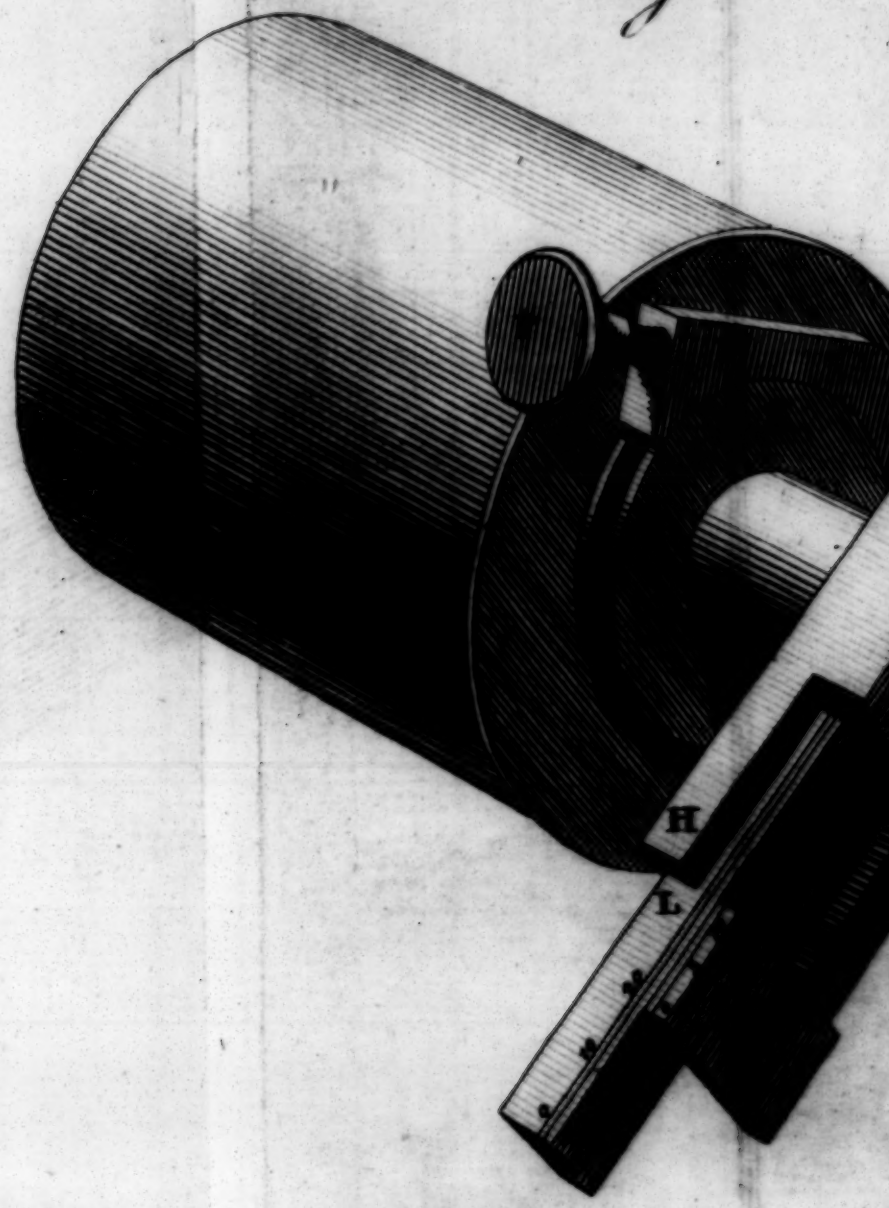
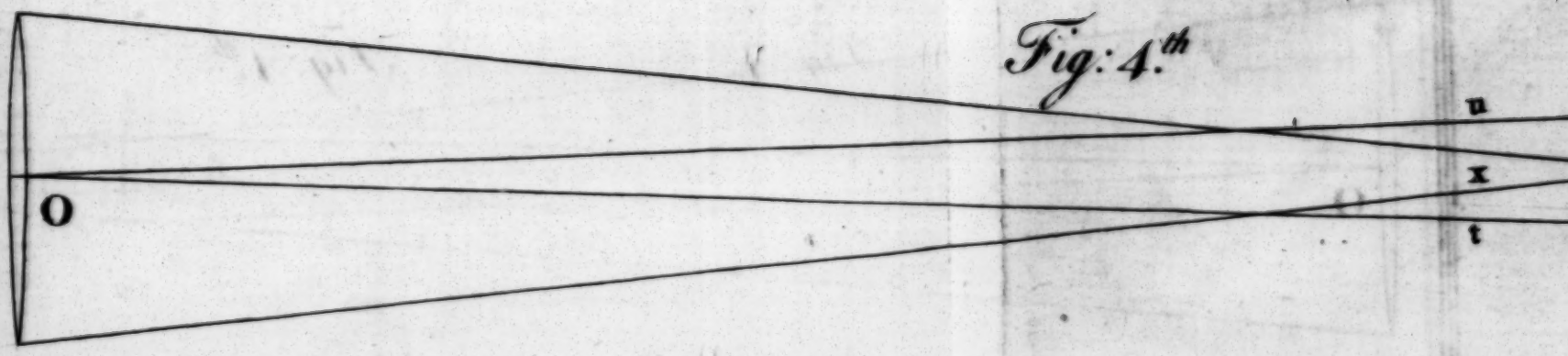
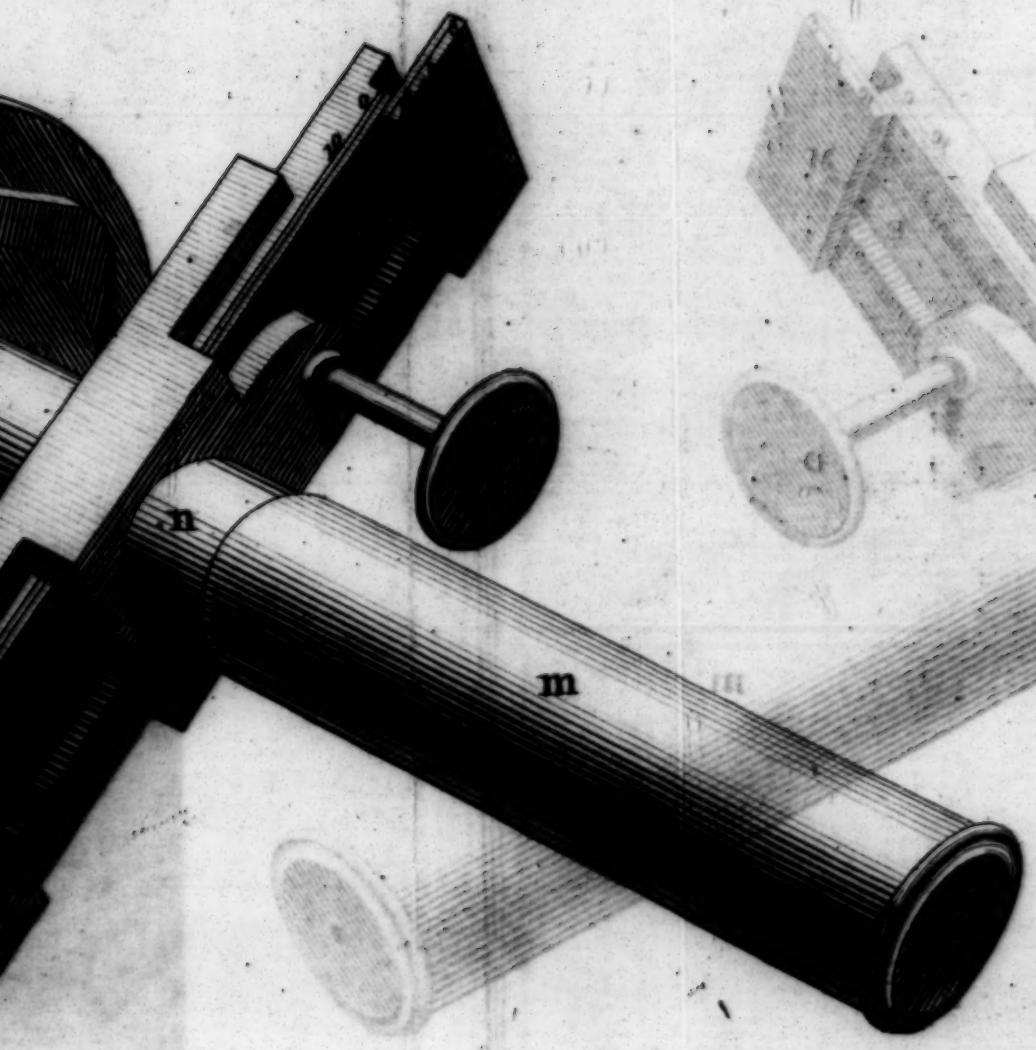
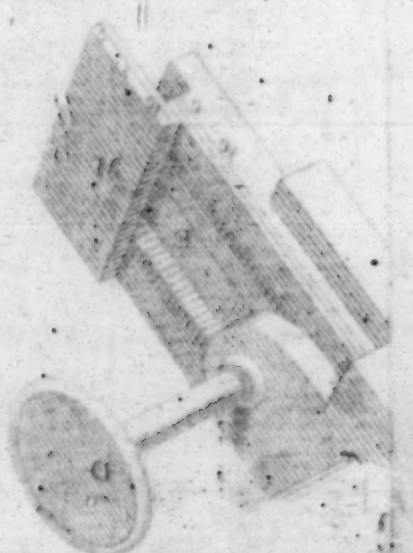


Fig: 4.th




n
m



a
g
m
b
t
y
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d

and, if the frame is moved towards the right, the relative motion of the two frames is ſhewn on the ſcale L by the vernier M; but if the frame B be moved towards the left, the relative motion is ſhewn by the vernier N.

This micrometer has a motion round the axis of viſion, for the conveniency of meaſuring the diameter of a planet, &c. in any direction, by turning an endleſs ſcrew F, and the inclination of the diameter meaſured with the horizon is ſhewn on the circle *g* by a vernier on the plate V. The teleſcope may be adjuſted to diſtinct viſion by means of an adjuſting ſcrew, which moves the whole eye tube with the micrometer nearer or farther from the object glaſs, as teleſcopes are generally made; or the ſame effect may be produced in a better manner, without moving the micrometer, by ſliding the part of the eye tube *m* on the part *n*, by help of a ſcrew or pinion. The micrometer is made to take off occaſionally from the eye tube, that the teleſcope may be uſed without it.

XXVIII. *Account of the Airs extracted from different Kinds of Waters; with Thoughts on the Salubrity of Air at different Places. In a Letter from the Abbé* Fontana, *Director of the Cabinet of Natural History belonging to his Royal Highneſs the Grand Duke of* Tuſcany, *to* Joſeph Prieſtley, *LL.D. F.R.S.*

Read April 16, 1779.

I TAKE the liberty of ſending you an account of ſome experiments which I made at Paris in the years 1777 and 1778 on the air extracted from various kinds of waters: ſome of the principal of which I thought proper to tranſcribe, that you might make uſe of them if you think that they are at all uſeful, and likewiſe lay them before the Royal Society.

I have extracted the air from the water of a well by means of common fire. The water was then made to boil in a large mattraſs of tin, which had a long tube of the ſame metal, which being bent into two different directions was with its extremity immerged in a tub of cold water. The mattraſs and its tube were intirely

filled

filled with water: the air which came out of it was received into three different veſſels. The air of the firſt veſſel, by being ſhaken in water, was diminiſhed a little; the air of the ſecond was diminiſhed of half its bulk, or rather more; and the air of the third veſſel was diminiſhed exceedingly. The *reſiduums* of air that remained unabſorbed were more or leſs phlogiſticated.

Another time I obtained almoſt entirely fixed air, excepting a little which remained unabſorbed by water, and was partly phlogiſticated.

A third time the air of the water of a well, obtained as above, was made to paſs through mercury into a tube anointed with oil of tartar, and it occaſioned a cryſtallization juſt like that which the pureſt fixed air is uſed to do.

A fourth time I impregnated with this air a quantity of common water, which abſorbed its own bulk of it, and became by theſe means acidulous, exactly like water with the pureſt fixed air. This water turned the tincture of turnſole red, and precipitated the lime in lime water. Another time a light was ſucceſſively extinguiſhed, and a bird died inſtantly in this air.

The water of the river Seine, filtrated through ſand, as it is drunk at Paris, was treated in the ſame manner as the water of the well. The air extracted from that water was half abſorbed by water, when ſhaken in it; the

remainder,

remainder, when treated with the teſt of nitrous air, gave II−4, II+1 (a); when the common atmoſpheric air treated with the ſame nitrous air gave II−4, II+8. It was therefore ſenſibly better than the atmoſpheric air, which, during three years of experiments made at Paris, I have conſtantly found to be inferior to the air of the Seine water, extracted as above.

Having repeated the experiment, I received the air into two different receivers. The firſt of which, by being ſhaken in water, was diminiſhed in the proportion of ten to ſeven; and by the teſt of nitrous air gave II−14, II+1, III+1, when the common air gave II−12, II+6, III+6.

The ſecond quantity of air was diminiſhed in the proportion of three to one; and when examined by the teſt of nitrous air gave II±0, III±0. From whence it may be concluded, that the firſt air was better than the atmoſpheric air; whereas the ſecond was worſe, and mixed with much fixed air.

Being in doubt whether the tin veſſel employed in the experiment above mentioned might not alter the nature of the air, &c. I made uſe of glaſs veſſels. Having therefore filled one of theſe veſſels, having a long neck bent in two directions, with the Seine water, I

(a) See p. 343. for an explanation of this meaſure.

obtained ſome air which ſeemed not ſenſibly diminiſhed when ſhaken in water. Having introduced one meaſure and 37 parts of this air into the tube uſed to try the diminutions, it gave with the nitrous air I+19, I+48, when the ſame quantity of nitrous and atmoſpheric air gave II+26, II+6: it is therefore certain, that the air extracted from Seine water is purer than common air.

Another time I extracted, in the ſame manner, and from the ſame water, the air; one meaſure and 24 parts of which being introduced into the tube, &c. and ſhaken, was reduced to one meaſure – 31 parts, that is, one fifth of it was abſorbed. Treated with the nitrous air it gave I-4, when equal meaſures of common and nitrous air gave I±0: it was therefore better than common air.

A third time I extracted the air, in the manner above mentioned, from the water of the river Seine, contained in three mattraſſes; this air was about one twenty-eighth of the bulk of the water, and it gave with the teſt of nitrous air II–14, II–9, III–9; when the common air mixed with nitrous air, as uſual, gave II–14, II+8, III+8. It is therefore clear, that the air extracted from the Seine water, by the action of fire in glaſs veſſels, is much better than common air, or than the air which is extracted from the ſame water when boiled in tin veſſels.

Another

Another time I filled a glaſs retort, which had a long and doubly bent neck, with Seine water. The water weighed about three pounds. The air that came out of it loſt a quarter of its bulk by being ſhaken in water; and afterwards being tried with nitrous air it gave II–16, II–16, III–16, when common and nitrous air gave II–12, II+12.

This experiment being repeated, the air was diminiſhed of one quarter by being ſhaken in water. One meaſure – 16 parts of this air introduced into the meaſuring tube gave II–32, II–2, when common air and the nitrous gave II–28, II+4.

The water d'Arqeuil at Paris is conſidered as very pure. I filled the tin veſſel above mentioned with it, and received the air that came out of it into three veſſels. Being ſhaken in water, the firſt of them was diminiſhed one fifth; the ſecond, three fourths; and the third $\frac{15}{16}$ by the operation in water. A light burned with a flame, more luminous than in common air, in the firſt air after it had been ſhaken in water. This air being tried with the nitrous air gave II–10, II–10, III–10. The ſecond gave II–10, II–17, III–30, when common and nitrous air gave II–2, II+14, III+14. The third air, before it was ſhaken in water, cryſtallized with the oil of tartar like fixed air. An equal bulk of it was abſorbed by

water, which by this mixture became acidulous; it precipitated the lime in lime water, extinguiſhed a light ſeveral times, and killed an animal inſtantly. It is therefore partly fixed air, and partly air which is not only better than common air, but likewiſe than that extracted from Seine water, even when this laſt has been boiled in glaſs veſſels.

The experiments being repeated with the ſame water of Arqueil, but in glaſs veſſels, the air obtained, after being ſhaken in water, was much better than that obtained from the ſame matter, when boiled in veſſels of tin.

I have alſo extracted the air from diſtilled water in glaſs veſſels, and having ſhaken one meaſure – 32 parts of it in water, it was reduced to one meaſure – 35 parts. With the teſt of nitrous air it gave I–6, when equal parts of common and nitrous air gave I–2, which ſhewed that it was better than common air.

I extracted the air again in the manner above deſcribed; but it was not ſenſibly diminiſhed when ſhaken in water. Two meaſures – 49 parts of it, with the teſt of nitrous air, gave I–2, I+8, when common air, &c. gave I+1, I+18: It is therefore better than common air.

I extracted the air from a great quantity of diſtilled water in the uſual manner, and found that it did not ſen-

ſibly diminiſh in water. With nitrous air it gave II−14, II−25, II+25, when common and the ſame nitrous air gave II−14, II+10, III+10; conſequently it was dephlogiſticated air, *viz.* purer than the air of the Seine and Arqueil, which are much better than common air.

I had the curioſity to try, whether any difference would ariſe from boiling diſtilled water in a matraſs of tin inſtead of glaſs veſſels; and found, that the firſt air was diminiſhed one tenth by being ſhaken in water, and afterwards with the nitrous air gave II−13, II−16, III−18, when common air gave II−12, II+8; which ſhews that it was dephlogiſticated air, but not ſo good as that extracted from the ſame water when boiled in glaſs veſſels. The ſecond quantity of air was not ſenſibly diminiſhed in water, and with nitrous air gave II−13, II−20, III−30; that is, it was more dephlogiſticated than the firſt.

The air extracted from diſtilled water is to that extracted from the water of the river Seine as 13 to 32 nearly; whence diſtilled water does not give more air than one ſixtieth of its bulk: but as the air extracted from the water of the Seine is half fixed air, it may be concluded, that the quantity of reſpirable air produced by both kinds of waters is nearly the ſame, and that they only differ a little in purity. It is however true, that other ex-

periments have ſhewn me that water in general abſorbs about twice as much of dephlogiſticated as of common air; for which reaſon, I think, that the reſpirable air of Seine water is rather leſs than that of diſtilled water. Accordingly I have found, that Seine water, after it has been boiled for a long time, abſorbs in forty days about one fourteenth of its own bulk of dephlogiſticated air, when in the ſame length of time it does not abſorb more than one twenty-eighth of common air. This ſeems to be an experiment of very great conſequence, and is much worth notice; eſpecially as it diſcovers a new characteriſtic by which dephlogiſticated air may be diſtinguiſhed from common air; and ſhews, that water abſorbs a greater quantity of thoſe kinds of air, which contain a leſs quantity of phlogiſton.

It muſt however be obſerved, that it is impoſſible to determine exactly the quantity of air that is extracted from veſſels filled with water, by means of fire; becauſe a portion of the air is abſorbed by the water of the tub in the act of its coming forth. It will certainly be more exact to receive the air in veſſels immerſed in quickſilver; but then there are many other inconveniencies to encounter.

It may be almoſt ſuperfluous to mention, that the above related experiments are very uſeful in explaining

the reaſon why ſome kinds of water have a peculiar ſharp taſte more than others; and eſpecially why ſome of them precipitate the lime in lime water, rendering it a calcareous earth, and change the tincture of turnſole into a red colour, as I have generally experienced with the well waters at Paris. We may alſo explain from hence, why ſome kind of waters can diſſolve iron, and keep it in diſſolution without depoſition; whereas other kinds of water are incapable of doing it, at leaſt do it much leſs than the pureſt diſtilled water. This is ſoon diſcovered by boiling the water, which will then depoſit the iron which before was diſſolved.

It will be ſufficient, for the preſent, to mention, that I have not only extracted from waters the different kinds of air they contained naturally, but have likewiſe made various experiments upon waters deprived of air, which, being expoſed, have again imbibed the atmoſpherical air, as I hinted above. I have determined the quantity and quality of thoſe airs. In general, I may ſay, that diſtilled water, deprived of air, imbibes again an equal quantity of air of the ſame kind as that it had loſt, and that in leſs than fifty days. Other kinds of water do the ſame, but with this difference, *viz.* that the air they abſorb, after being boiled, is better than that they have loſt; and in this particular they come very near to the nature of diſtilled water itſelf.

If the waters deprived of air are expoſed to common air held in receivers in contact with quickſilver, the air which remains unabſorbed is ſo much more phlogiſticated as a leſs quantity of it remains in the receiver. This experiment deſerves conſideration.

By means of pure water, eſpecially diſtilled water, common air may be changed into dephlogiſticated air, that is, into air much more ſalubrious than the beſt common air which we breathe; and this, for what I know, is the only means of meliorating common air: for all the artificial methods (great numbers of which I have tried) have proved either uſeleſs or noxious, but never ſuch as promiſed to be of any great utility to human kind.

Though I have long thought of applying thoſe experiments to ſome uſe for the purpoſes of life, the want of time and a proper apparatus has hitherto hindered my doing any thing; I now begin to be in hopes I ſhall be able to do ſomething. In the mean time I think it of ſome importance to have it known, that water not only poſſeſſes the property of diminiſhing the noxious part of tainted air, but has alſo the power, and that in a very high degree, of dephlogiſticating common air; which muſt certainly be one of the methods by which nature keeps the atmoſphere in a ſtate conſtantly fit to ſupport animal life, it being certain, that the water in various

circumſtances

circumſtances muſt loſe either a part or the whole of that air which it hath abſorbed from the atmoſphere.

It may with ſome reaſon be ſuſpected, that in my experiments of extracting the air from water by the action of fire, the air might be conſiderably altered by the vapour of the water itſelf. As this difficulty was of ſome force, I endeavoured to remove it in the following manner. I introduced into a tube, through water, a quantity of common air of known goodneſs, and I cauſed the ſteam of water boiling in a matraſs, from which the air had been previouſly extracted, to paſs through it. The heat of the ſteam ſometimes made the water occupy above five times the ſpace it did when cold; yet the air ſo treated was not at all altered by it, as appeared by the teſt of nitrous air. The event of the experiment, although repeated various times, was conſtantly the ſame.

I muſt obſerve, laſtly, that having once cauſed the air of boiling water to paſs into receivers filled with, and ſtanding in, quickſilver, I found that the air was better than uſual. I have obſerved the ſame thing when I have cauſed the air to go through diſtilled water into receivers filled with it: which obſervation, if the event of the experiment is conſtantly the ſame, induces me to believe, that the air loſes ſome of its good properties by going through water not very pure; or, which ſeems to be

rather

rather more probable, that a quantity of air leſs good is, by the action of the vapours and the heat, extricated from that impure water, and is mixed with the air that comes out of the matraſs; from whence this air is debaſed.

I muſt not omit to mention a new character of equal importance with that which diſtinguiſhes the dephlogiſticated from the common air. This new character has been equally unknown, and deſerves the attention of philoſophers, becauſe at the ſame time it diſcovers a new property of the atmoſpherical air, which I ſhould never have ſuſpected if experience had not offered it to me.

I have found, that common air ſhaken in water, inſtead of being diminiſhed is ſenſibly increaſed in its bulk. The increaſed ſpace is in proportion to the time the air is ſhaken in water, and it begins to be ſenſible even from the beginning, that is, after a few ſeconds. This augmentation I have ſometimes brought to be one twelfth of the bulk of the air, and even more: it muſt, however, be confeſſed, that I met with great variety in the experiments of this kind made at different times. After that the bulk of the air ſhaken in water is increaſed to a certain degree, it then begins to decreaſe continually; and, in proportion to this decreaſe, the air becomes gradually leſs good. When the experiment is tried in cloſe veſſels, the diminution

nution cannot be observed; but I shall reserve, for another opportunity, to speak of the laws and causes of those diminutions and augmentations, and the differences observed between common and other kinds of air, and when the experiments are tried in water, &c.

For the present I shall only mention, that if the dephlogisticated air be shaken in the tube, in the manner above mentioned, not only it does not increase its bulk, but it begins to diminish from the very beginning of the operation, and it continually loses more and more of its bulk, and with its bulk of its purity.

This last mentioned property of the dephlogisticated air seems to shew, that this is a fluid much different from common air, because it has its peculiar properties by which it differs from common air not from more to less only, but entirely; as is shewn by the property this fluid has of being absorbed by water; whereas common air receives an increase of bulk and elasticity by being shaken in water.

All that I have been saying above, in order to give an idea of my method, and the words I use to express the diminutions made by the mixture of nitrous air and other kinds of respirable air, is not sufficient to obtain results constant and certain, so as to deduce any consequences from them. Even after that all the elements are

corrected,

corrected, and all the cauſes of error hitherto unknown or neglected by the moſt diligent obſervers, are avoided (which cauſes ſenſibly alter this kind of experiments) it is abſolutely neceſſary to follow always a conſtant and equal method, not only in the act of introducing the various kinds of air into the tube, but alſo after the mixing of the two kinds of air. The leaſt variation of circumſtances cauſes very great variations in the reſults of the experiments, and theſe variations of circumſtances are ſo minute that I never ſaw any of the perſons that obſerved my experiments who could diſcover them, although apprized of my deſign. The neglect alone of this uniformity of operation may occaſion an error of from 20 to 50 parts and upwards in the experiment with common air; but with dephlogiſticated air the error is incomparably greater, ſo much that the ſame quality of air a moment after may decompoſe even a double quantity of nitrous air; ſo that the pureſt common air would appear to be noxious, and phlogiſticated air; and the dephlogiſticated air would appear leſs good, and even noxious: for, by the teſt of nitrous air, it might appear little different from a mixture of dephlogiſticated and phlogiſticated air.

I ſhall take another opportunity to ſpeak of all the particulars relative to my method; but for the preſent I

muſt mention, that, after uſing all the caution I am maſter of; the greateſt error in the diminution is not greater than one ſixtieth of the common air introduced in the tube: ſo that, after having made five or ſix experiments ſucceſſively, the probable error is ſo ſmall that it may be ſafely neglected. But, if one choſe to operate upon a quantity of air nine or ten times greater than that I commonly uſe, the error could not be 1000th part of the quantity of the air, which quantity would not be more than a few cubic inches. In the deſcription of my method I ſhall alſo mention the means by which I obtain nitrous air of a nearly equal and conſtant goodneſs, and in what manner I can refer my experiments to ſome conſtant ſtandard. For want of this method it is that we are not certain of the obſervations made about the ſalubrity of common air in different places, and that no tables of its changes have been made.

I have not the leaſt heſitation in aſſerting, that the experiments made to aſcertain the ſalubrity of the atmoſpherical air in various places, in different countries and ſituations, mentioned by ſeveral authors, are not to be depended upon; becauſe the method they uſed was far from being exact, the elements or ingredients for the experiment were unknown and uncertain, and the reſults very different from one another.

When

When all the errors are corrected it will be found, that the difference between the air of one country and that of another, at different times, is much leſs than what is commonly believed, and that the great differences found by various obſervers are owing to the fallacious effects of uncertain methods. This I advance from experience; for when I was in the ſame error, I found very great differences between the reſults of the experiments of this nature which ought to have been ſimilar; which diverſities I attributed to myſelf rather than to the method I then uſed. At Paris I examined the air of different places at the ſame time, and eſpecially of thoſe ſituations where it was moſt probable to meet with infected air, becauſe thoſe places abounded with putrid ſubſtances and impure exhalations; but the differences I obſerved were very ſmall, and much leſs than what could have been ſuſpected, for they hardly arrived to one fifteth of the air in the tube. Having taken the air of the hill called Mont Valerien at the height of about 500 feet above the level of Paris, and compared it with the air of Paris taken at the ſame time and treated alike; I found the former to be hardly one thirtieth better than the latter. In London I have obſerved almoſt the ſame. The air of Iſlington and that of London ſuffered an equal diminution by the mixture of nitrous air; yet the

air of Islington is esteemed to be much better. I have examined the air of London, taken at different heights, (for instance, in the street, at the second floor, and at the top of the adjoining houses) and have found it to be of the same quality. Having taken the air at the iron gallery of St. Paul's cupola at the height of 313 feet above the ground, and likewise the air of the stone gallery which is 202 feet below the other; and having compared these two quantities of air with that of the street adjoining; I found, that there was scarce any sensible difference between them, although taken at such different heights.

In this experiment a circumstance is to be considered, which must have contributed to render the above mentioned differences more sensible; this is, the agitation of the air of the cupola, for there was felt a pretty brisk wind upon it, which I observed to be stronger and stronger the higher I ascended; whereas in the street, and indeed in all the streets I passed through, there was no sensible wind to be felt. This experiment was made at four in the afternoon, the weather being clear. The quicksilver in the barometer at that time was 28,6 inches high, and FAHRENHEIT's thermometer stood at 54°. After having related all these circumstances, it will be necessary to give the mean result of all the various ex-

periments

periments made upon each of thoſe quantities of air treated after my method with the nitrous air. The air of the ſtreet gave II−13, II+6; the air of the ſtone gallery, which was 202 feet high, gave II−14, II+5; and the air of the iron gallery, which was 313 feet high, gave II−14, II+5. The reſults of the two laſt experiments are exactly the ſame; and that of the firſt is hardly at all different from them. Mr. CAVALLO, who has ſhewn the literary world his ability in examining nature, aſſiſted me in thoſe experiments, ſo that a miſtake can hardly be ſuſpected. From this we clearly ſee, how little the experiments hitherto publiſhed, about the differences of common air, are to be depended upon. In general I find, that the air changes from one time to another; ſo that the differences between them are far greater than thoſe of the airs of different countries, or different heights; for inſtance, I have found that the air of London, in the months of September, October, and November, 1778, when treated with the nitrous air, gave II−6, II+15, which is a mean reſult of many experiments which differed very little from each other. The 26th day of November laſt, I found the air for the firſt time much better, for it gave II−12, II+12; but the 14th of February laſt, the air gave II−18,

11–18, 11–7; from whence it appears, that the air of the 14th of February was better than it had been for ſix months before. There can be no doubt of the accuracy of the experiments, becauſe I compared the air taken at different times with that which I had firſt uſed in the month of September, and which I had preſerved in dry glaſs bottles accurately ſtopped. Now if the formulæ expreſſed above are compared together, it will be found, that the difference between the firſt terms is of twelve parts, and that between the latter of ſeven; that is, of one tenth and one twenty-fourth of the whole quantity of air: which are much greater differences than thoſe mentioned above. Notwithſtanding this, I could not perceive any particular change of health, or facility of breathing, ariſing from thoſe changes of the ſalubrity of the atmoſpherical air; and I am informed, that no particular diſeaſes appeared which could indicate any remarkable change of air.

Nature is not ſo partial as we commonly believe. She has not only given us an air almoſt equally good every where and at every time, but has allowed us a certain latitude or a power of living and being in health in qualities of air which differ to a certain degree. By this I do not mean to deny the exiſtence of certain kinds of noxious

in ſome particular places; but only ſay, that in general the air is good every where, and that the final differences are not to be feared ſo much as ſome people would make us believe. Nor do I mean to ſpeak here of thoſe vapours and other bodies which are accidentally joined to the common air in particular places, but do not change its nature and intrinſical property. This ſtate of the air cannot be known by the teſt of nitrous air, and thoſe vapours are to be conſidered in the ſame manner as we ſhould conſider ſo many particles of arſenic ſwimming in the atmoſphere. In this caſe it is the arſenic, and not the degenerated air, that would kill the animals who ventured to breathe it.

In this place, therefore, I do not mean to ſpeak of thoſe changes which do not immediately alter the nature of the air itſelf. The other ſtates of that fluid are of another kind, and they are not to be examined by means of nitrous or inflammable air (the uſes of which laſt, I ſhall ſhew on another occaſion). The ſame thing may be ſaid of thoſe vapours or particles which may be good for reſpiration, and do not change the nature of the air. Some vegetables, for inſtance, can diffuſe through the air ſuch exhalations as may be of real uſe to the animal œconomy when they

are

are breathed for a long time, to imbibed by the pores of the skin. I remember to have often put various flowers, as roses, pinks, &c. in veſſels full of common air confined by quickſilver, where I left them for ſeveral hours, after which time I found, that the air was not at all altered, but that various animals ſeemed to breathe it very well, notwithſtanding that the flowers filled the greateſt part of the veſſels. On the contrary, I have found, that the vapours ariſing from lime ſlacked in water, either do not alter the air at all, or very little; though when breathed with the air they occaſion the death of animals.

I would not have any body ſuppoſe, that I think it of little importance to know the goodneſs of the atmoſpherical air, and the changes it undergoes. On the contrary, I believe it to be a very uſeful inquiry, for mankind, becauſe we do not yet know how far one kind of air more than another may contribute to a perfect ſtate of health; nor at what time ſmall differences may become very conſiderable, when one continues to breathe the ſame kind of air for whole years, eſpecially in ſome kind of diſeaſes. An exact method of examining the goodneſs of common air may even be uſeful to poſterity, in order to aſcertain whether our atmoſphere degenerates in a length of time. This curious inquiry, together

with

with the methods, are the production of this eighteenth century; and our descendants must have some gratitude for the philosophers who found out, as well as for those who improved it. If our ancestors had known and transmitted it to us, we should, perhaps, at present be able to judge of one of the greatest changes of our globe, of a change which very nearly interests human life!

XXIX. *Account of ſome Experiments in Electricity. In a Leter from Mr.* William Swift *to the Rev. Dr.* King, *F. R. S.*

Read April 29, 1779.

SIR,

Greenwich, April 28, 1779.

BEING encouraged by the attention the Royal Society did me laſt year to give to the account of ſome electrical experiments I had made, which you were ſo good as to preſent to them, I take the liberty to beg their indulgence again, and hope you will be pleaſed to lay before them ſome new improvements I have made in my electrical apparatus. I ſhall eſteem myſelf very happy if what I have done is worthy the farther attention of that moſt learned and reſpectable body.

One particular addition I have made to the apparatus conſiſts in what I call an anti-conductor: it is exactly like the prime conductor, but it is fixed to the cuſhion of the machine, and conſequently, when the cylinder is put in motion, the anti-conductor is charged negatively, that is, the electric matter is diminiſhed therein in the ſame pro-

portion

portion as it is increaſed in the prime conductor in the ſame time.

Another thing peculiar to this machine is, that the whole is inſulated, ſo that being able to collect the electric matter without any connection with the earth, and having at the ſame time bodies or conductors poſitively and negatively electrified, I apprehend I am enabled by this apparatus to exhibit many experiments more analogous to the natural effects of lightning from the clouds, than it is poſſible to do with only one conductor poſitively electrified; becauſe in nature clouds are conſtantly flying in the air which are differently electrified, and, diſcharging themſelves in each other, produce the lightning often ſeen in the atmoſphere.

I have annexed a ſlight ſketch of the apparatus: *a* is the glaſs cylinder; *b* the prime conductor, on two glaſs ſtems; *c* the anti-conductor, on two glaſs ſtems, connected with the cuſhion of the machine; *dd* is a pole or round ſtaff, well covered with metal, with a ball at each end, which hangs over the two conductors *b* and *c*; this ſtaff is faſtened to the croſs pole *f*, which is ſuſpended by a ſilk line in ſuch a manner that the ſtaff *dd* is equally balanced.

It may be proper to mention a few common experiments and obſervations, to ſhew, that the two conductors

are charged and differently charged, that is, the one positively or *plus*, the other negatively or *minus*, as soon as the cylinder is put in motion by turning the wheel; though, perhaps, this will not be doubted by any person conversant in electrical experiments, unless they have been fully perſuaded, that the electric matter cannot be excited without connecting the machine with the earth; which is not the caſe in this apparatus, for the glaſs cylinder and both conductors are inſulated.

1. When the cylinder moves, and a body approaches the prime conductor, ſuch body will draw a ſpark from that conductor at the ſame diſtance, and conſequently of the ſame length as will be drawn from the approaching body by the anti-conductor. And a pith ball is equally attracted by both, which ſufficiently ſhews, that both conductors are charged or electrified.

2. The following common experiments will ſhew, that they are differently electrified.

I take a wire with a ſmall piece of cocoa wood, about one inch and a half long, pointed, faſtened to one end of the wire, and connecting the other end to the anti-conductor; as ſoon as a conducting body approaches it there is a bright ſpark reſembling a ſtar, which appears to ſettle upon the end or point of the wood; but when the wire is connected with the prime conductor, there iſſues

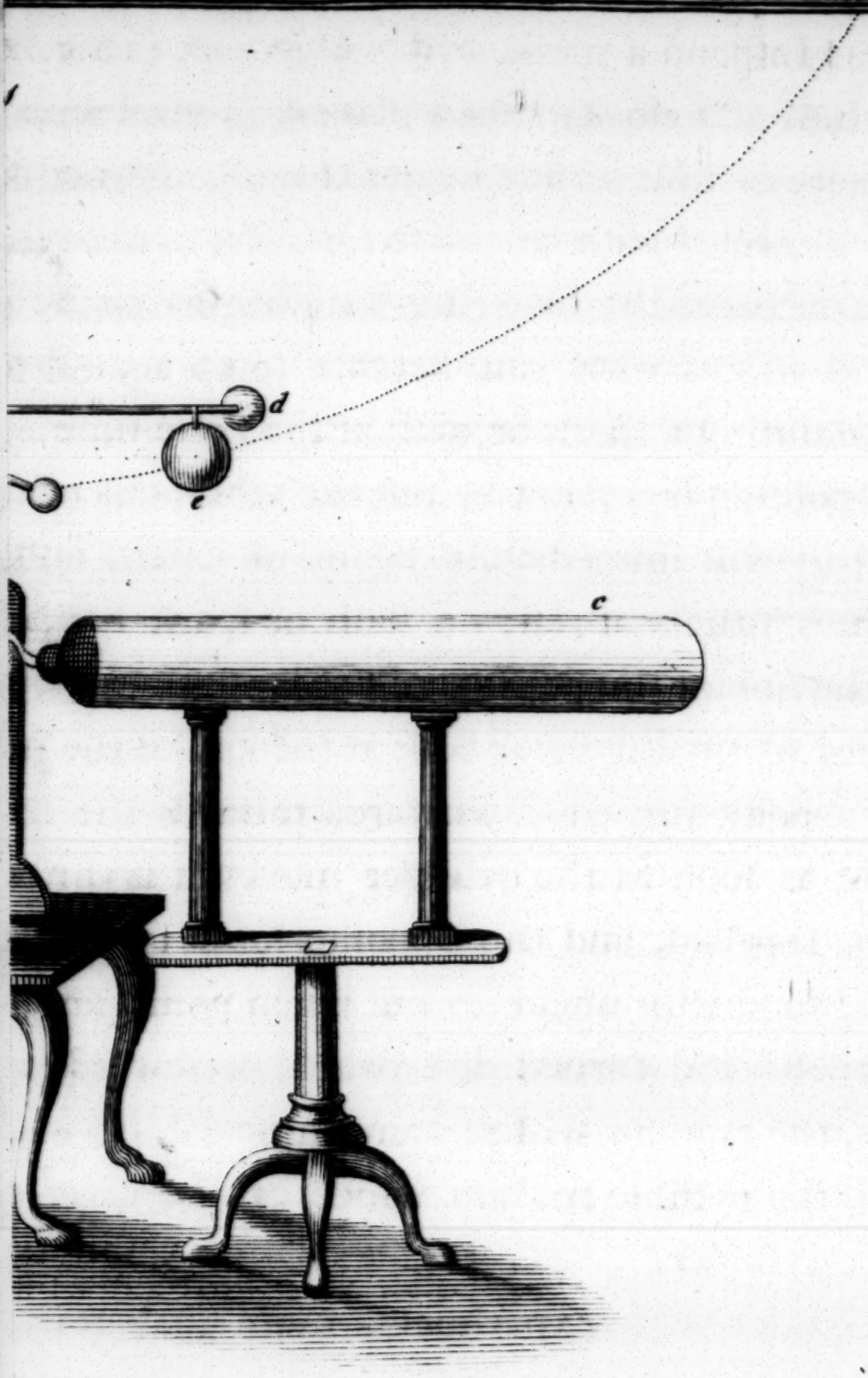

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Fig: 1

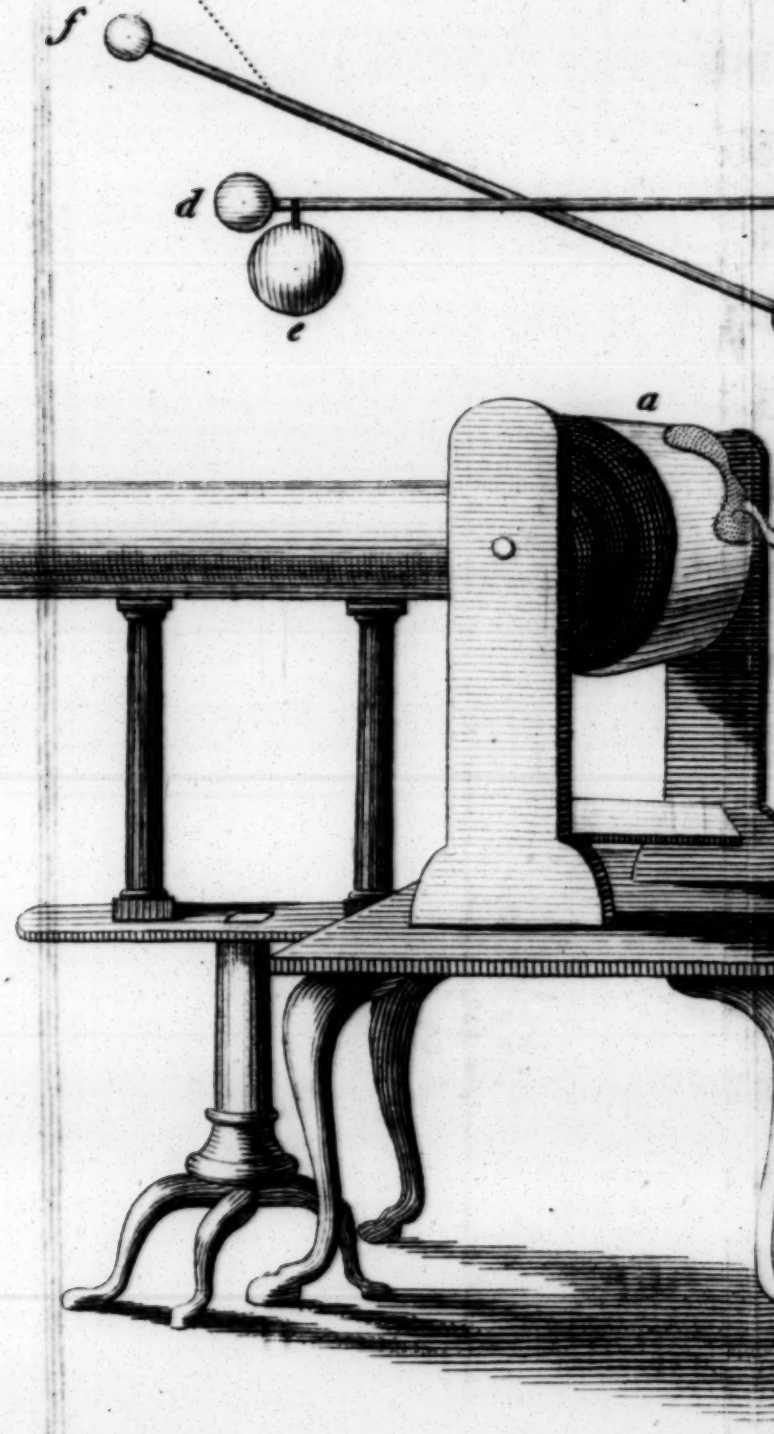
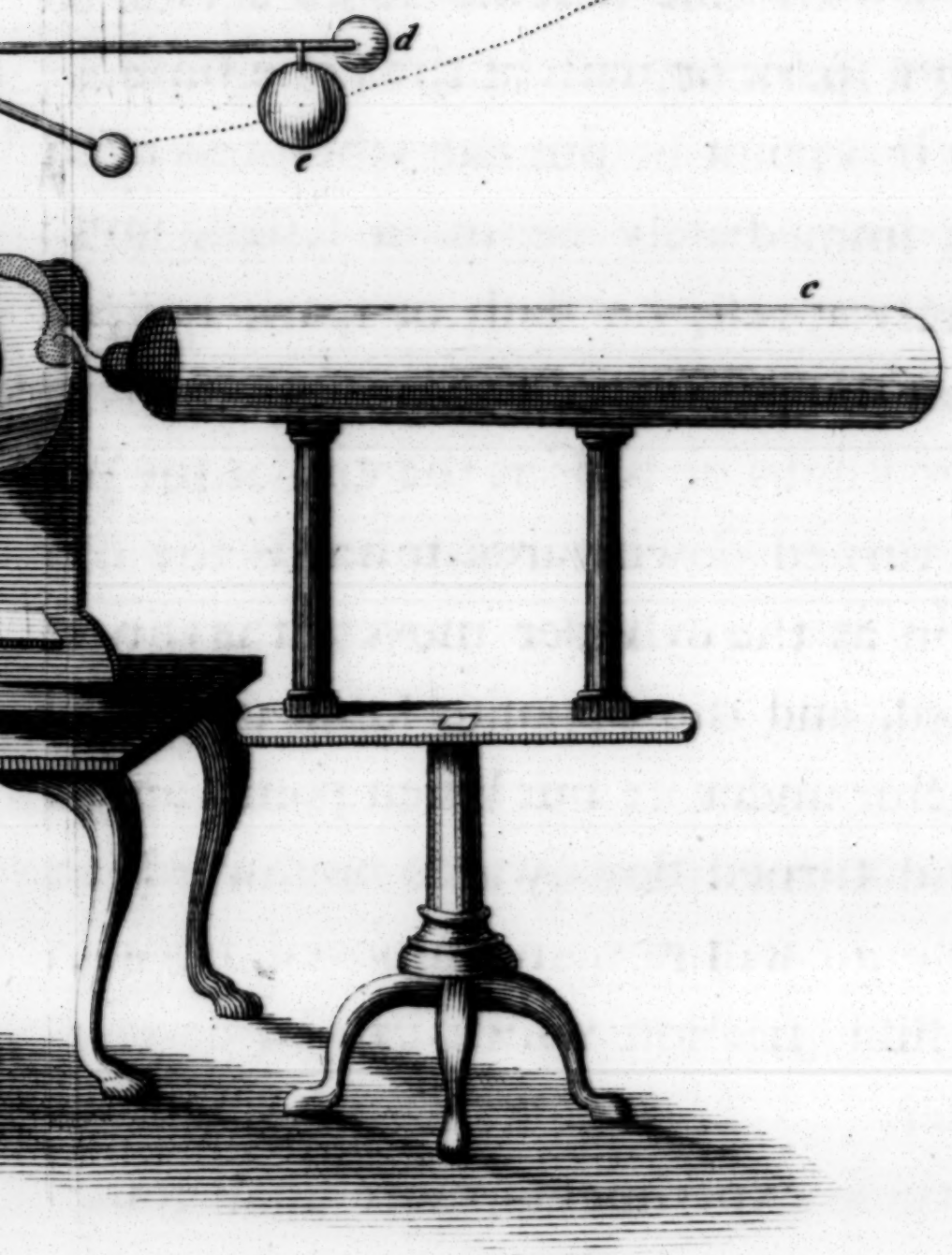


Fig. 1



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from the end of that wood a pencil of rays diverging to the point towards the approaching body, which, in appreheads, demonstrates the conductors to be differently electrified.

3. It may also be seen by another experiment. Take two jars, coated as in the Leyden experiment, and charge one by the prime conductor, the other by the anti-conductor; the first will be positively, and the second negatively electrified; which is proved by applying a discharging rod to the balls connected with the inside of the jars, when both immediately discharge themselves, which they would not do, if both jars were charged from the same conductor.

These experiments I only mention, to shew, that the two conductors are both electrified, and with this difference, that the one has more, the other less, electrical matter than in its natural state.

4. When the pole *dd* is let down by slackening the silk line on which it hangs within the sphere of action of the two electrified conductors, and being equally balanced remains *in equilibrio* over both, as soon as the cylinder begins to turn, the pole vibrates regularly towards each conductor, and as it approaches the one or the other gives a flash or spark.

In

In this ſtate if a ball be preſented to either conductor, it makes no alteration; the pole continues to vibrate as before, even though a flaſh comes on the ball: but when a point is preſented to either of the conductors *b* or *d*, the vibrations of the pole begin to abate, and no flaſh comes to the point. And as ſoon as any connection is made with the earth, the point being preſented to one conductor, the pole attaches itſelf to the other with continual flaſhes.

5. I take two glaſs globes coated *ee*, and ſuſpend them towards the ends of the pole *dd*. On turning the cylinder the pole will vibrate, and the globes will, in a ſhort time become charged; each globe will have its outward coat charged in the ſame way as the conductor over which it hangs, as will appear by diſcharging them into each other with a common diſcharging rod. This experiment ſhews the poſſibility of compreſſing the electric matter, though the globes are perfectly inſulated.

6. While the machine remains in this ſtate, let the prime conductor *b* be connected with other clouds, fig. 2. which are made to move over houſes having conductors terminated with points; the globe *e* over the prime conductor will be thrown upwards or repelled, and the other globe only will receive a charge from the anti-conductor *c*: or if, on the contrary, the anti-conductor *c* be

connected with the flying clouds, the oppoſite globe only will receive its charge from the prime conductor. But if balls inſtead of points be made to terminate the conductors of the houſes, the globes *e e* will vibrate till both be equally charged.

7. Or if we take off the globes and place a large jar or battery to each conductor, and charge them by turning the cylinder till the electrometer riſes to ninety degrees, the ſtrokes of the vibrations of the pole *d d* become very ſtrong; then, by means of an inſulated rod, I place a point on the upper part of either conductor where the knob of the pole ſtrikes, and no exploſion will come on that point. But if, inſtead of a point, a ball be ſo interpoſed, there will be an exploſion; and the larger the ball is, the greater the exploſion will be. In the latter part of this experiment, when the ball is interpoſed, if any perſon applies his finger to any wire on the board *w w w*, or to the ſolid pieces of marble ſtanding on the board, he will be ſenſibly affected with a ſhock every time the exploſion happens; but when the knob is in contact with the point in the former part of the experiment, as there is no exploſion, neither will his finger be affected on the marble or any part of the wire.

8. Another experiment, which ſeems to agree in effect with all the foregoing, is as follows.

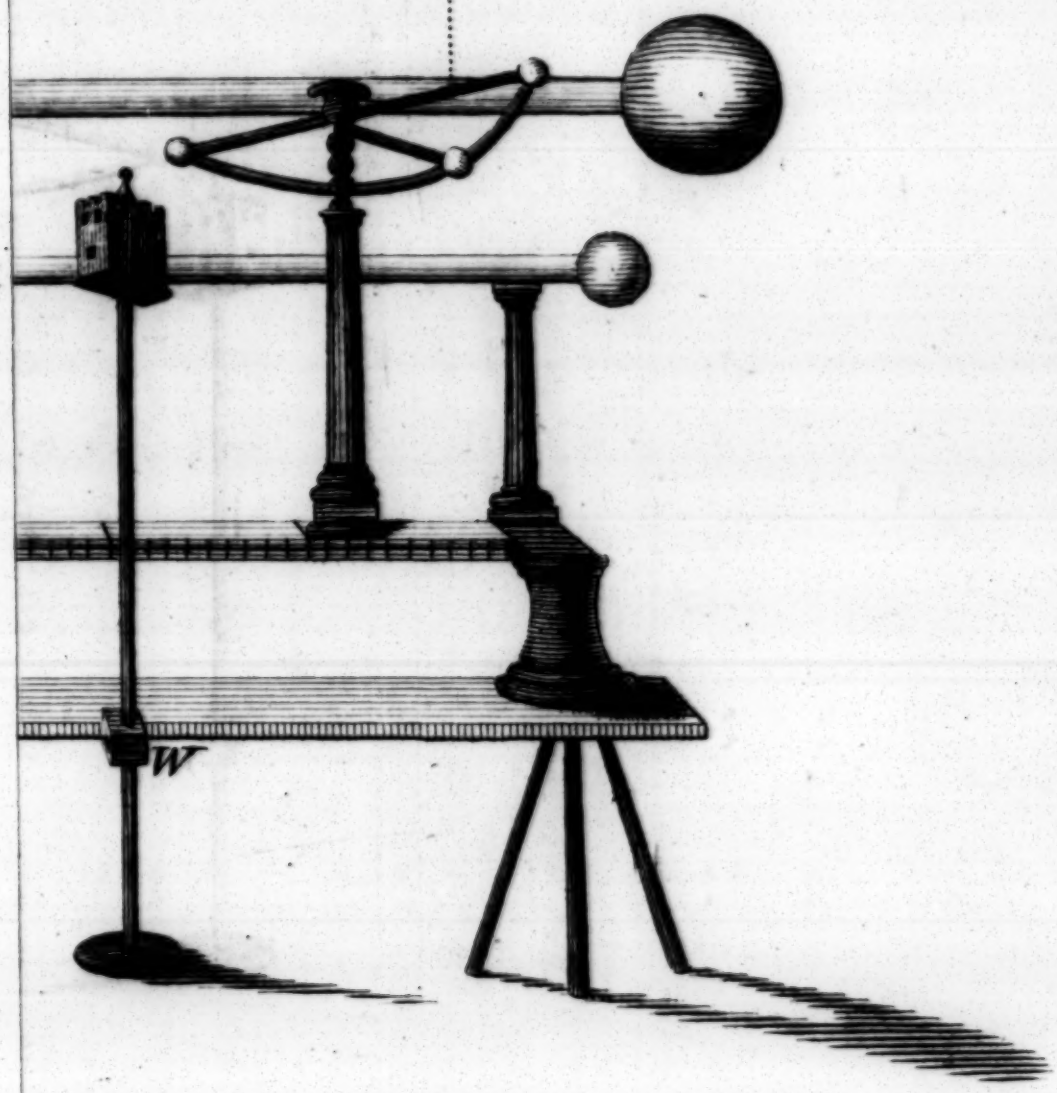
From

From either the prime conductor, or from the anti-conductor, I ſuſpend a metal ball which can ſwing like the pendulum of a clock; I then place on a level with it, at the diſtance of four or five inches from it, another ball fixed and connected with the earth; as ſoon as the cylinder begins to turn, the ſwinging ball begins to be attracted, and ſtrikes with conſiderable force againſt the fixed one, emitting a ſpark or flaſh at the ſame time.

In ſuch poſition if a point be put the vibrations of the ſwinging ball will immediately begin to leſſen, and it ſoon becomes intirely at reſt, no flaſh or ſpark happening from the inſtant the point is put there.

9. On one of the knobs or balls at the end of the pole *dd*, I put a point turned downwards towards the conductor, and as ſoon as the cylinder moves it is thrown upwards or repelled, and the oppoſite knob or ball adheres to the conductor under it; but when points are put on both knobs and turned downwards or towards the conductors, the pole *dd* will remain unmoved, notwithſtanding all the poſſible friction which can be given by turning the cylinder.

10. To render theſe experiments more analogous to the natural phenomena of lightning and rain deſcending from the clouds, I place a veſſel of water inſulated, and as the clouds (fig. 2.) being charged paſs along the frame,



BASIRE. Sc.

Fig.

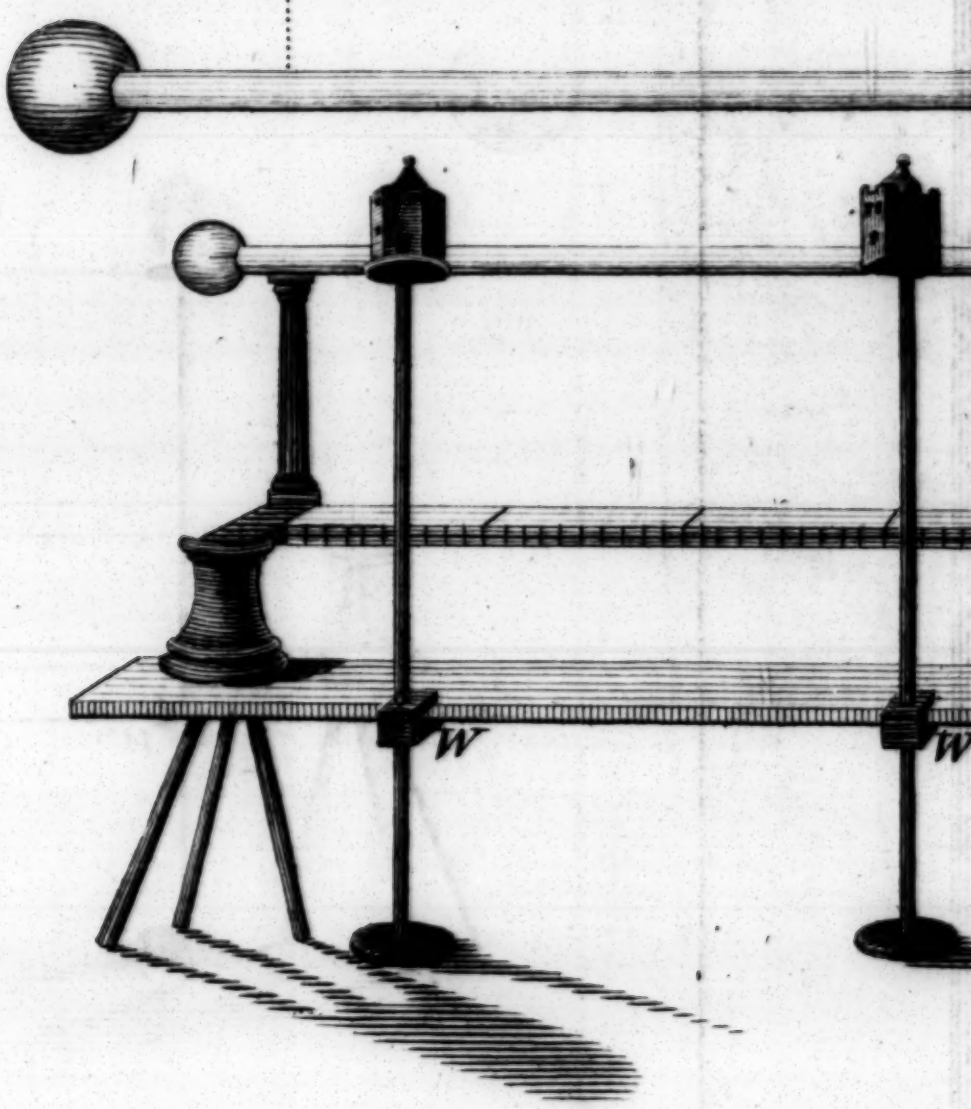
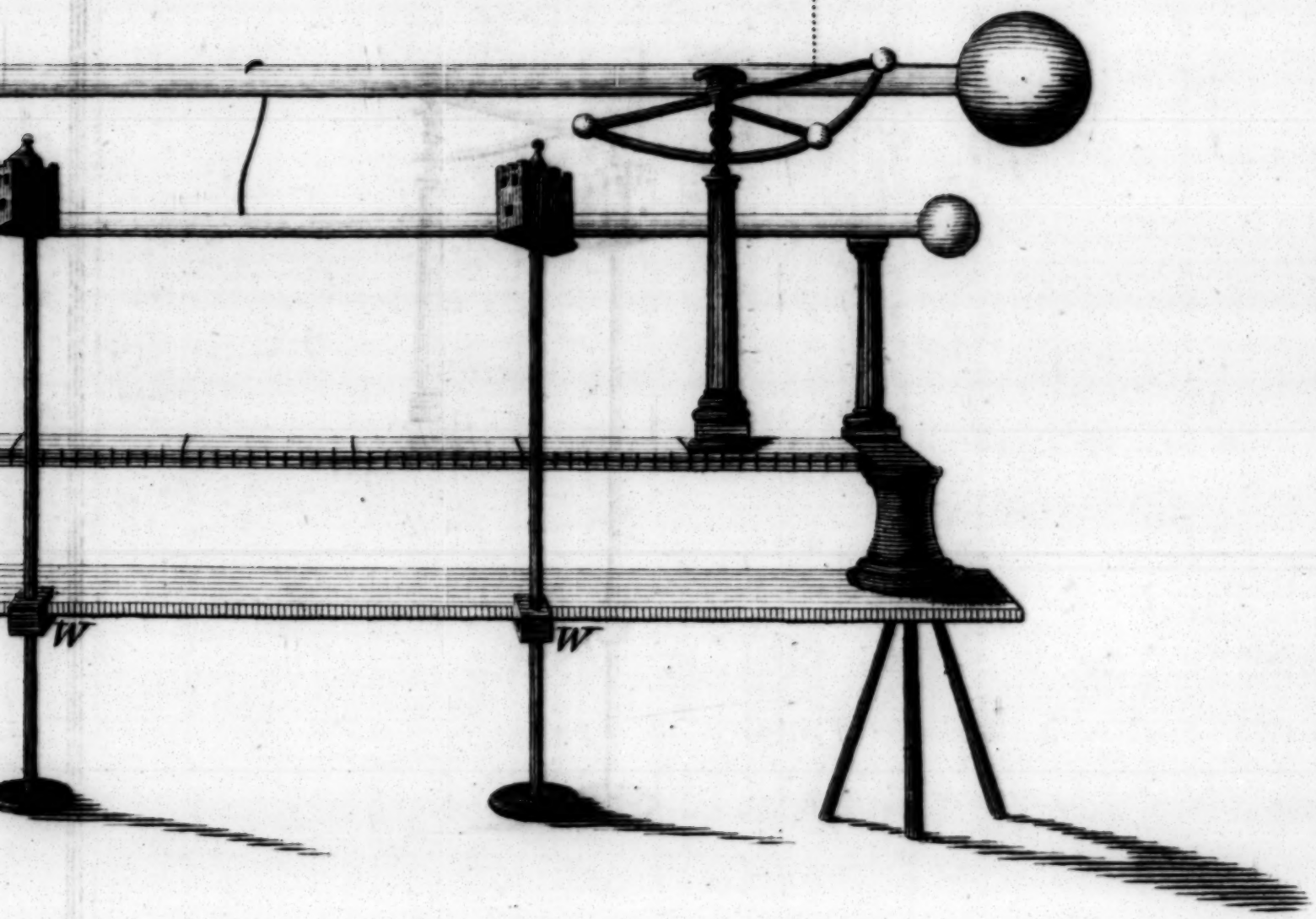


Fig. 2



BASIRE, Sc.

frame, they revolve over another cloud, which is introduced in the place of the middle houſe: all the houſes being taken away through this cloud, the water in the inſulated veſſel is made to paſs and to deſcend in a ſtream. When a ball is interpoſed between this cloud and the revolving ones, there will be frequent flaſhes upon it; but when, inſtead of a ball, a point is put there, the electric matter paſſes off gradually and ſilently without any flaſh.

Thus, SIR, I humbly apprehend, the whole current of theſe experiments tends to ſhew the preference of points to balls, in order to diminiſh and draw off the electric matter when excited, or to prevent it from accumulating; and conſequently the propriety or even neceſſity of terminating all conductors with points, to make them uſeful to prevent damage to buildings from lightning. Nay the very conſtruction of all electrical machines, in which it is neceſſary to round all the parts, and to avoid making edges and points which would hinder the matter from being excited, will, I imagine, on reflection, be another corroborating proof of the reſult of the experiment themſelves.

I am, &c.

XXX. Sitodium *inciſum et macrocarpon, uſuſque fructuum qui exinde naſcuntur, deſcripta a* Carolo Petro Thunberg, *M. D.; communicated by* Joſeph Banks, *Eſq. P. R. S.*

Read May 13, 1779.

AUCTORES, qui deſcriptionem dederunt hujus arboris vel fructus ejus, quantum ſcio, pauci ſunt.

RHEEDE, HENR. in Horto Malabarico, Amſt. annis 1678 et ſeq. fol. *macrocarpon* deſcribit.

RUMPHIUS, GEORG. EVERH. M. D. et medicus Amboinenſis annos ab hinc octoginta, utriuſque ſpeciei hiſtoriam dedit, quæ invenitur in Herbario Amboinenſi, a Prof. LAUR. BURMANNO, annis 1741 ſeq. publici juris facto.

ZANONIUS, JACOB. in Hiſtoria ſtirpium rariorum, Bonon. 1742, fol. *macrocarpon* memorat.

HAWKESWORTH, JOHN, Voyages round the World, Lond. 1773, vol. III. 4°, *inciſi* mentionem facit.

ELLIS, JOHN, fructum hujus deſcripſit anno 1775, in a Deſcription of the Mangoſtan and the Bread-fruit, Lond. 4°.

FORSTER,

FORSTER, JOH. REINH. et GEORG. in Characteribus generum Plantarum, p. 101 et 102. tab. 51. Lond. 4°. 1776, deſcriptionem genericam et figuram dederunt.

Ego, anno 1775, Bataviæ Indorum commoratus, arbores haſce characteribus botanicis deſcripſi et ad genus ſecundum ſexuale ſyſtema reduxi, quæ deſcriptio ſub nomine Radermachiæ inciſæ et integræ in actis Stockholmienſibus, anno 1776, p. 250. invenienda eſt. Eodem anno, 1775 ſcilicet, ſemina *S. macrocarpi* miſi ad hortum medicum Amſtelodamenſem. Poſtea redux e Japonia, anno 1777, plantas minutas vivas utriuſque ſpeciei plantatas ad eundem hortum e Batavia tranſmiſi. Deinde in Europam anno 1778 ipſe revertens inſignem numerum mecum duxi e Ceilona, tam plantarum utriuſque ſpeciei vivarum, quam alterius ſpeciei ſeminum, quæ pluribus modis conſervare ſtudui, ſcilicet:

1°, In chartulis, quæ theca includebantur, et aliquando aeri in umbra exponebantur.

2°, In lagenis vitreis bene obturatis.

3°, In cera.

4°, In arena ſicca. Ultimæ hæ duæ methodi optimæ fuerunt. Præterea

5°, Sub ipſo itinere ſingulis menſibus ſemina terræ mandavi, ut ſenſim excreſcere poſſent ſub vario cœlo et vario anni tempore.

SYNONYMA varia, quibus inſignitur hoc genus a variis nationibus et variis in locis, ſunt ſequentia:

Cingallis, Herreliga.

Mallabaris, Plápallam, Tsjakamaram, Pilau.

Malais, Succong, Tjampeda, Tsjampedaha, Nanca.

Portugallis, Jaka, Jaccas, Jaqua, Jaqueira.

Hollandis, Syrſack, Schoofack, Schrootſack, Broodboom.

Anglis, Bread-fruit Tree.

Botanicis, Soccus, Saccus, Radermachia, Artocarpus, Sitodium.

Deſcriptio generica.

FLORES MASCULI.

Cal. nullus. *Spadix* cylindricus vel ſubclavatus, ſenſim incraſſatus, floribus undique tectus.

Cor. *Petala* duo, oblonga, concava, obtuſa, villoſa, filamentum includentia.

Stam. *Filamentum* unicum intra ſingulam corollam, filiforme, diaphanum, corollæ longitudine.

Anthera pyramidalis.

FLORES FEMINEI.

Cal. nullus. *Pericarpium* ovatum, germinibus tectum.

Cor. nulla.

Piſt.

Piſt. *Germina* convexa, frequentiſſima, ſexangulata.
Stylus filiformis, perſiſtens.
Stigma unum vel duo, capillaria, revoluta, lineam longa.

Peric. *Bacca* ovata, muricata, carnoſa, multilocularis.
Semina multiplici ſerie imbricata, ovata, oblique triquetra, carne molli involuta.

Character genericus. ♂ Cal. o. Corol. dipetala.
♀ Cal. o. Corol. o. Styl. 1.
Bacca multilocularis.

Habitatio inſularis ut in Ambona, Banda, et aliis Moluccis, Sumatra, Java, Ceilona, Maldivicis aliiſque.

Deſcriptio ſpecifica.

1. *S. inciſum*, foliis inciſis; ramulis floriferis.

Soccus lanoſus. Soccus capas. RUMPH. Herbar. Amboin. P. I. pag. 110. tab. 32.

Soccus granoſus. Soccun bidji. RUMPH. Herb. Amb. P. I. pag. 112. tab. 33.

Soccus ſilveſtris. Soccun utan. RUMPH. Herb. Amb. P. I. pag. 114. tab. 34.

Bread-fruit. HAWKESWORTH's Voyages, vol. II. p. 80. tab. N° 11.

Radermachia

Radermachia inciſa. Ego Act. Stockholmenſ. anno 1776, pag. 253.

Malais. Tjampeda, Nanca bibulang, Succong, Succon radja, Succon timbul.

Hollandis, Bataviæ. Schoorſack, Syrſack, Broodboom.

Ceilonæ. Syrſack, Maldiviſche Syrſack.

Creſcit ſpecies hæc ſponte in inſulis Ambon. Banda, et aliis Moluccis, in Java in et circum urbem Bataviam ſatis vulgaris, in inſulis Maldivicis alibique; culta in Mallabaria ut Surattis, ad promontorium Comorin; in Coromandelia ut Sutucorin, Tranquebariæ, Nagapatnam; inque Ceilona circum Colombo, Galæ, Maturam, Jafnam, Trinquilimale, in inſulis Marianis, &c.

Floret fructuſque maturos profert, bis in annum, primis octo menſibus.

Caulis arboreus, erectus, craſſitie hominis, altitudine quinqueorgyali. *Rami* faſtigiatim oppoſiti, ſubverticillato-quaterni, patentes. *Ramuli* ſubverticillato-quaterni, floriferi.

Folia alterna, petiolata, oblonga, ſupra medium profunde inciſa, 9-lobata, integerrima, villoſo-ſcabrida, patentia; ſupra viridia nervis pallidis; ſubtus pallida; bipedalia, pedem lata; juniora plicata, minora, viſcida. *Petiolus* craſſus, ſubtriqueter, villoſus, pollicaris. *Stipulæ* juniora folia involventes, duæ, ſeſſiles, lanceolatæ,

acuminatæ, concavæ, integerrimæ, intus glabræ, extus hirſutæ, deciduæ, palmares.

Flores in ultimis ramulis, maſculi et feminei diſtincti in eodem ramulo, pedunculati.

Pedunculus ſubteres, villoſus, erectus, bipollicaris, craſſitie digiti.

Spadix ſpithamæus, cernuus, deciduus.

Pericarpium magnitudine capitis infantis, ſterile vel ſeminibus ſterilibus farctum.

2. *S. macrocarpon*, fol. indiviſis trifidiſve; caudice ramiſque floriferis.

Tsjakamaram. REEDE, Hort. Malabar. P. III. pag. 17. tab. 26, 27, 28.

Saccus arboreus major. Nanca. RUMPH. Herb. Amboin. P. I. p. 104. tab. 30.

Saccus arboreus minor. Tsjampadaha. RUMPH. Herb. Amboin. P. I. pag. 107. tab. 31.

Jaca. ZANON. Hiſt. Stirp. rarior. pag. 127. tab. 90, 91.

Radermachia integra. Ego Act. Stockholm. anno 1776, pag. 254.

Malais. Tsjampedaha, Nanca, Nanca buburi.

Portugallis. Jacca ſive Jaccas.

Hollandis. Syrſack, Scoorſack, Schrootſack, et Broodboom proprie.

Creſcit.

Creſcit ſponte in inſulis Moluccis, Ambon, Banda, aliis; in Java parcius, Sumatra, Ceilona vulgaris, Coromandelia, Tranquebaria, inque promontorio Comorin.

Floret fructuſque bis in annum, primis octo menſibus, ſenſim profert.

Caulis arboreus, erectus, craſſitie humana, quinqueorgyalis et ultra. *Rami* alterni, patentes, ramuloſi. *Ramuli* alterni, iterum ramuloſi, villis longis rigidis hirſuti.

Folia alterna, petiolata, ovato-oblonga, obtuſa cum acumine obtuſo, obſolete ſerrata, indiviſa, nervoſa; ſupra læte viridia, læviſſima; ſubtus pallidiora, pilis rigidis hirſuta; patentia, ſpithamæa. *Juniora* evidenter dentata ſunt, qui dentes in adultis evaneſcunt. *Petiolus* ſubtriqueter, glaber, pollicaris. *Stipulæ* ut in priori. Raro folium unum alterumve inciſum.

Flores maſculi et feminei in caule vel ramo eodem diſtincti. *Pedunculus* vel ſimplex vel ramoſus, pendulus, pollicem craſſus, pedalis. *Pedicelli* tres, quinque vel plures, digitum craſſi longique.

Spadix digitalis, erecto-patens.

Pericarpium magnitudine inſigni, pondere 30 librarum et ultra, fertile, adeo ut omnium baccarum facile maxima hæc ſit. *Semina* magnitudine tripla et quadrupla Amygdalarum, ſæpe 200 uſque 300, ovato-oblonga,

altera

altera extremitate acuta, altera obtusa, parum complanatis lateribus.

Observationes in genere circa utramque speciem.

Arbor tota, rami fructusque abundant succo lacteo albo, fibroso et tenacissimo, qui sine oleo ablui non potest.

Crusta extima fructuum coriacea est, tuberculis undique hispida et muricata; hac ablata semina in conspectum veniunt, quæ carne et variis tunicis circumvolvuntur. Primum inter semina deteguntur lamellæ albæ, carnosæ, tenues, lineares, semina distinguentes. Deinde singulum semen circumvolvitur tunica carnosa, striata, immatura alba, matura flava, sapida, molli. Postea observantur duæ tunicæ tenues et seminibus magis propriæ, quarum exterior alba et siccatione secedens, interior brunnea; hæ duæ conservari debent in seminibus plantandis. Media columna in fructibus parvis valde crassa est, quo vero magis fructus accrescunt et maturantur, eo hæc tenuior evadit.

Utriusque speciei fructus, dum maturus est, odoris paululum ingrati et fere nauseosi est, licet interiora delicata sint.

Si pluviæ cadant tempore floreſcentiæ, pericarpia parva putrefacta decidunt et parca tunc erit meſſis; ſi vero ætatem quindecim dierum dudum attigerint, pluviæ vix amplius nocent.

S. inciſum longe vulgarius eſt inſula Java, *macrocarpon* vero in Ceilona, ubi illi præfertur tam ob gratiorem ſaporem, quam ob inſignem ſuam magnitudinem.

S. macrocarpon propagatur ſeminibus, quæ facile excreſcunt; *inciſum* vero, quod ſterile eſt, radicibus ſub ipſa ſuperficie terræ plantatis. *S. inciſum* optime colitur tam in humo arena mixta quam in terra argillacea; *macrocarpon* vero amat argillam humo mixtam.

Flores fructuſque primos *S. inciſum* profert tertio quartove anno, *macrocarpon* vero anno quinto vel ſexto: utrumque perennare dicitur annos 100 uſque 150, ſemper fructiferum. *S. inciſum* fructus ſuos profert in ipſis extremitatibus ramulorum, *macrocarpon* vero in ipſo trunco et majoribus ramis.

Obſervationes circa S. inciſum.

Species hæc longe communiſſima eſt in orientalibus regionibus Indiæ Orientalis. In Ceilonam, Mallabariam, et Coromandeliam allata fuit et radicibus propagata. In inſulam Ceilonam primum ab incolis Maldivicarum inſularum, qui quotannis naviculis adveniunt, annis 1727

vel

vel 1728 allatæ fuerunt radices et plantatæ intra Caſtellum juxta templum in horto domus proximæ. Duas has arbores proceras ipſe vidi, e quibus in Ceilona plurimæ aliæ, magna copia propagatæ ſunt. Ob hanc rationem hodie adhuc ab Hollandis arbor Maldivica (*Maldiviſche Syrſack*) appellatur.

Radices plantatæ primum poſt ſpatium duarum, trium vel plurium menſium mihi inceperunt prima germina emittere, licet ſub ſirio ardente et intra tropicos plerumque navigans qualibet fere veſpera aqua non pauca irrogaverim.

Semina hujus magnitude dupla piſi circumdantur carne tenuiſſima et delicata, ſed ſterilia ſunt nec plantata excreſcere volunt.

Obſervationes circa S. macrocarpon.

Radices amant ſuperficiem terræ ſiqué alte ſub terra impoſita ſepeliuntur, arbor emori dicitur, præcipue ab humo pingui.

Dum fructus maturare incipiunt, comeduntur a Sciuro palmarum atque Corvis, ſi non ab horum inſidiis præſervantur. Sciurus palmarum primus plerumque eſt qui vulnus infligit, et apertura ab illo facta adveniunt Corvi, illum fugantes quodque ſupereſt fructus comedentes.

Fructus varios trunci et ramorum maturos ponderavi, qui plerumque 30 et 35 librarum pondere fuerunt.

Narratum mihi fuit a variis fide dignis, quod ipſæ radices infra ſuperficiem terræ interdum fructus proferant, qui ſub maturatione terram divellunt et adeo ponderoſi ſæpe evadunt, ut a duobus ſervis portari debeant ſinguli, hique fructus maxime delicati æſtimantur.

Accidit interdum, ut fortioribus ventibus dejiciantur fructus ponderoſi, dum valde grandes e tenui et non ſatis forti pedunculo dependent. Dum valde grandes evadunt fructus et aliquot eidem inſident pedunculo ut tres, quinque vel plures, tunc plerumque corbes ex foliis cocos confectæ ſubponuntur, ne ab hoſtibus ſuis ſupra memoratis lædantur, nec pericarpia adhuc immatura, præ pondere rupto pedunculo, decidant et ne noceant invidi oculi, unde fructus decidunt, ut ſuperſtitioſe credunt.

Duobus vel ſaltem tribus menſibus fructus ſuos maturat hæc ſpecies, dumque primi fructus maturi ſunt, iterum florere incipit.

Ceilonenſes triplici nomine inſigniunt hos fructus, prout illis utuntur varia ætate et prout magis vel minus maturi ſunt. *Pollos* audiunt, dum adhuc parvi magnitudine ovi Struthionis, et immaturi ſunt, ætate dimidiæ vel integræ menſis; hoc quoque nomen tribuitur fructibus *S. inciſi* in genere. *Herreli*, dum ſemimaturi ſunt, magnitudine cocos unius cum dimidia. Caro ſeminum

tunc adhuc alba, lacteſcens eſt, nec ſine præparatione edulis. *Warreka*, dum plenam fructus maturitatem attigerunt; caro ſeminum mollis, dulcis, flava et ſine prægreſſa præparatione edulis et delicatiſſima eſt, licet dulcedo aliquantum nauſeoſa ſit. Maturitatem minorem vel majorem concludimus ex mollitie et ſono, quem digitis pulſatus fructus plus vel minus obtuſum edit. Pollos diſſectus in aëre rubeſcit et adſtringens eſt, ſed coctus iterum albeſcit. Diſtinguunt quoque nonnulli fructus in duplicem varietatem, ſcilicet nucleis duris et mollibus: e mollibus conficitur *Fios*.

Hæc ſpecies in Ceilona vulgatiſſima eſt et ſpontanea, Cingallis pauperibus, qui nihil vel parum oryzæ poſſident, maxime neceſſaria et inſignis utilitatis, dum fructum cum pauxillo raſuræ Cocos vel cum raſura Cocos et oryzæ parum comedunt.

Mos obtinuit in Ceilona ſemina hujus cum humo et aqua mixta in diſciſſo Cocos externo involucro ſerere, ut germinent, antequam in hortis plantentur.

Licet hæc ſpecies per totum fere Orientem creſcat, nullibi tamen locorum vel majores vel ponderoſiores fructus vel delicatiores profert, quam quidem in inſula Ceilona et quibuſdam Mallabariæ regionibus, ut ipſe non modo obſervavi oculatus teſtis, ſed et in antiquis peregrinatorum ſcriptis adnotatum inveni.

usus. *Lignum* paucorum annorum, nimis lacteſcens adhuc, ullius vix uſus eſt. Cum vero arbor 20 vel 30 et 40 annos attigerit, adhiberi poteſt lignum pro vario uſu in conſtruendis domibus, pro laminis, trabibus; in conſtruendis naviculis, ubi injurias aquæ et aeris 30 annos et ultra perdurat; in fabricandis ſellis, ſcamnis, ciſtis, cæteris.

Lac arboris inſervit pro capiendis pſittaccis et aliis avibus, dum inſtar viſci inungitur arborum ramis.

Fructus autem præprimis eſt, qui generi humano tantæ utilitatis eſt, vel ſterilis vel ſeminibus farctus.

Bacca, dum omnino matura eſt, ſcilicet *S. macrocarpi*, carnem habet ſemina obveſtientem, mollem, flavam, dulcem et delicatam, quæ ſine ulla præparatione prægreſſa vel ſola editur vel cum oryzæ pauxillo, et optime ſapit. Hæc caro in duas vel tres partes diſciſſa cum oleo recenti Cocos aſſata quoque comeditur.

Bacca utriuſque ſpeciei, dum adhuc immatura eſt, id eſt Pollos et Herreli, variis modis præparata nutrimento inſervit. Ingreditur præparationem Currii Caldu, Currii ſeco, Niembelæ, Lixationis cum lardo, Pollos tjundido, fricadellarum, Pollos aſſati, Empade, Pei et hinc, in futuros uſus, exſiccatur fructus.

Pauperes Cingalli ſequenti modo fructus e *S. inciſo*

ciso et e *S. macrocarpo* exemtis seminibus, præparatos adhibent. Omnia cultro in tenuissimas et minutissimas partes conscindunt, cumque Cocos raspato *aa* supra lapidem contrita vel sola comedunt vel cum additis Capsicis, sale et Allio. Mos quoque Hollandis in Indiis est, Pollos utriusque speciei in tenues lamellas concisum cumque oleo fortiter assatum comedere, juxta potum Theæ. Hæ lamellæ tenues aliquando etiam lixantur cum carne et jusculo gallinarum.

Nuclei vel cum tunica sua carnosa, vel cum toto fructu, vel soli pro nutrimento adhibentur ut in ferculis variis, fructu cocto, Pollos tjundido, confectione seminum, Fios placentulisque. *Nuclei* mundati etiam soli eduntur tam a ditioribus quam a pauperibus, cocti vel assati. Cocti plerumque a pauperioribus cum Cocos raspato et sale comeduntur. Ditiores interdum hisce in quatuor plerumque partes concisis et passulis mixtis vel porcos juniores vel anseres assatos, loco Castanearum, implere sciunt. Usus, quos habent medicales nuclei, tanti quidem non sunt; nihilo tamen minus Javani exinde cum lympha Cocos emulsionem parant, quam Diarrhœis laborantibus propinant.

Omnibus itaque prioribus, quæ homini alicujus utilitatis esse possunt, pro nutrimento et alimento adhibitis, cætera tamen ut inutilia non abjiciuntur, ut cortex fructus

externus,

externus, lamellæ paleaceæ, quæ ſemina circumſtant, tunicæ nucleorum et columna media; ſed hæc in minutas partes conſciſſa et cocta, ſuibus, qui avide devorant, apponuntur.

E columna media diſciſſa, parum cocta et in aceto macerata Atjar conficiunt. Atjar (Pickles) vocant omne, quod aceto forti conſervatur et pro condimento ciborum inſervit. Ex pollos quoque Atjar fit, dum antea decorticatus et coctus aceto inditur.

Deſcriptio fercularum, quas e fructibus præparant Hollandi *in* India *Orientali, præſertim in inſula* Ceilona.

1. *Currii Caldu.* Rc. tenuium lamellarum Pollos M ij. Coque parum cum aqua et radice Curcumæ ut flaveſcant. Adde Piſcis ſiccati (Comlimas dicti, e Maldivicis inſulis adducti) et contuſi P ij. ſi velis; lactis Cocos ſecundi ℔ ij. Coque iterum tenui igne per ſemihoram, aliquando agitando.
2. *Curii ſeco.* Rc. tenuium lamellarum Pollos M ij. Coque cum aqua et radice Curcumæ parum, ut flaveſcant. Adde Cocos uſtæ contritæ ſimul cum ſeminibus Coriandri et piperis, cortice cinnamomi, mace et aquæ ſalſæ parum: lardi ſaliti cocti et in parva fruſta conciſi q. vis: aquæ, in qua coctum fuit lardum, parum.

Omnia

Omnia hæc bene miscuntur et adduntur lactis Cocos cochlearia aliquot. Coque per dimidiam horam. Postea alio vase assatur butyro herba allii concisa, priora omnia adduntur beneque miscuntur. Additur porro succus duarum limonum, miscetur optime et paululum aquæ salsæ ad gratum saporem additur. Quidam addunt unicum Capsicum et duos allii bulbos.

3. *Pollos Tjundido.* Rc. Pollos vel Herreli cum vel sine nucleis tenue consciforum q. vis. Coque primum cum aqua et radice Curcumæ. Adde Cocos raspatæ cum Capsico, allii bulbis concisis et sale M ij. Coque iterum leni igne massam fere siccam, sæpe agitando et interdum vase clauso. Coctione per dimidiam horam peracta, paratum est.

4. *Currii Castaneum.* Rc. Nucleorum a tunicis mundatorum et in longis angustis lamellis conciforum circiter M ij. Coque parum cum aqua et radice Curcumæ, ut flavescant. Adde, si velis, Piscis Comlimas dicti P j: lactis Cocos secundi ℔ß–1: Allii herbæ concisæ parum. Coque vase clauso per dimidiam horam leni igne, interdum agitando. Adde lactis Cocos primi paululum. Coque iterum, continuo agitando, per $\frac{1}{8}$ horæ partem.

5. *Niembela.* Rc. Pollos vel Pollos et Herreli valde grosse conciforum q. vis. Adde aquæ salitæ et allii foliorum

conciſorum parum: lactis Cocos primi ℔j. Piſcis (Comlimas dicti) contuſi ℔ ij. Coque leni igne, ſæpe agitando, per dimidiam horam.

6. *Lixatus cum lardo fructus.* Rc. Pollos et Herreli groſſe conciſorum q. v. Adde aquæ ſalitæ et allii herbæ conciſæ parum: lardi ſalſi cocti et in fruſta conciſi atque aqua frigida abluti q. vis: aquæ, in qua lardum coctum fuit, paululum: macis et fruſtulorum Cinnamomi, herbæ allii conciſæ et cinnamomi pulveriſati *aa* ℔ ij. Coque per quadrantem horæ, aliquando quaſſando.

7. *Fructus coctus.* Rc. nucleorum cum carne et tunicis maturorum, inque duas vel tres partes conciſorum q. v. Coque cum aqua et radice Curcumæ q. s. addendo ſub coctione paululum ſalis. Ita coctus comeditur a pauperibus, cum raſura nuclei Cocos; ditiores addunt Capſicum et piſces ſiccatos.

8. *Lamellæ Pollos aſſatæ.* Rc. lamellarum tenuium Pollos coctarum q. vis: lactis Cocos ℔ ß: farinæ ℳ ij. Hiſce mixtis involve lamellas et in vaſe plano aſſa cum olei Cocos recentis magna copia.

9. *Empade.* Rc. lamellarum Pollos ut ſupra aſſatarum q. v. Adde herbæ allii tenue conciſæ, lactis Cocos (Comlimas) piſcis ſiccati, bulborum allii conciſorum, et pulveris Cinnamomi. Coque vaſe plano, leni igne,

continuo cum cochleari lac Cocos ſuper affundendo. Per quadrantem horæ ita coctis adde allii bulborum tenue conciſorum et cum butyro aſſatorum, quantum lubet.

10. *Fricadellæ ex Pollos.* Rc. Pollos cocti et in pultem redacti M ij: allii herbæ conciſæ, Cinnamomi, Piperis, nucis moſchatæ, ſalis, panis biſcocti *aa* pulveriſatorum q. vis: vitellum ovi unicum. Miſce optime fiantque inde boli, obvolvendi albumine ovi, ut melius cohæreant. Poſtea aſſentur butyro ſolo vel oleo Cocos et butyro ſimul, uſque dum rufeſcant. Paratis ſic bolis ſuper affunditur embemma e butyro, pulvere cinnamomi, pipere, ſale et ſucco limonum confectum.

11. *Confectio ſeminum et carnoſæ ejus tunicæ.* Rc. Tunicarum carnoſarum ſeminum in duas vel tres partes conciſarum q. v. Aſſa cum oleo expreſſo recenti Cocos. Oleum deterge cum linteo vel pone aſſatas tunicas in cribro, ut defluat oleum. Poſtea obvolve ſyrupo ſacchari condenſato, ſicca et lagenis bene obturatis conſerva. Per tres menſes ita conſervari poſſunt et eduntur juxta potum Theæ.

Rc. Seminum a tunicis mundatorum et integrorum. Cum oleo aſſata involve ſyrupo ſacchari, ſicca et ut priores conſerva in eundem uſum. Si ſemina aſſata

cum ipso syrupo in lagenis obturatis includuntur, per dimidium annum conservari possunt.

12. *Fios.* Rc. seminum maturorum q. v. involve pulti e lacte Cocos, farina et vitello ovi confectæ. Assa cum oleo recenter expresso Cocos.

13. *Placentulæ.* Rc. Vespere Syri dicti (i. e. aceti e succo Cocos fermentato): farinæ q. v. unde fiat bolus. Altero mane, nocte peracta fermentatione, adde lactis Cocos, farinæ seminum siccatorum q. s. et ovorum vitella pro lubitu. Fiant modo consueto placentulæ.

14. *Pei* sive *Jambal.* Rc. Pollos cocti, seminum sinapis *aa* ℳ ij, radicis Curcumæ q. v. Omnia seorsim in pultem redacta, optime misceantur cum paululum aceti. Adhibetur cum aliis cibis, cum piscibus et cum oryza quandoque sola, ob gratum saporem, præsertim pauperibus. Quidam his quoque addunt Capsicum, zingiber, allium et sal commune, ut priora in pultem redacta.

15. *Siccatio fructuum.* Rc. fructus semimaturos vel integros vel discissos exemta carne. Coque parum et postea sicca optime insolatione, et ita serva vel suspensos in Camino vel alio sicco loco. Hac ratione per annum conservari possunt. Pauperes ita siccatos cum Cocos rasura comedunt vel etiam coctos. Dum aliquando magna copia colligunt fructus, ita illos siccatos in futuros usus conservant.

Observationes

Obſervationes circa fercula præparanda.

Currii, quod communiſſimum Indianis eſt ferculum, juſculum quoddam audit, cui additur radix Curcumæ ut flavum induat colorem, et capſicum, ut igneus plus minus evadat ſapor, præter pauca alia aromata. Ejuſmodi juſculum fit e juſculo carnium ut gallinarum, anſerum, anatum, ovium, piſciumque; item ex emulſione nucleorum Cocos, cum carnibus ſupra dictis vel cum carnibus et variis ſimul fructibus conciſis ut melonibus, bilimbing (Averrhoa Bilimbi) aliis, vel etiam cum hiſce et aliis fructibus abſque carne. Dum juſculum adeo paucum eſt, ut fere ſiccum, vocatur *Currii ſeco*.

Omne, quod Currii audit, ingredi debet radix Curcumæ, ut color evadat flavus et ad ſaporem acrem plus minus igneum, Capſicum et alia aromata, pro cujuſque lubitu, adduntur ut Currii Caldu, Currii ſeco, Currii caſtaneo et Pollos tjundido.

Radix Curcumæ, quæ communi nomine in Indiis appellatur *Borriborri*, et ad finem Currii additur, ſequentem in modum præparatur. Primo aqua maceratur ut mollis fiat, dein ſupra lapidem planum cum pauxillo aquæ conteritur in pultem ſinapis denſioris conſiſtentiæ. Lapis, plerumque *lapis Borriborri* dictus, planus eſt, plus

vel minus magnus, cum piſtillo lapideo, ea longitudine ac ipſa latitudo lapis, cylindrico, altera extremitate rotundato, altera tranſverſo. Super hunc lapidem in pultem teruntur non modo radix Curcumæ ſed etiam pollos coctus, ſinapis, Capſicum, allium, Cocos raſpatum, oryza toſta et alia aromata, &c.

Currii ſeco varia requirit, quæ antequam in pultem redigantur, toſta eſſe debent ut Cocos, ſemina Coriandri, cortex Cinnamomi et, ut quidam amant, oryza, piper, feniculum et cetera. Hæc ſingula vel ſeparatim vel aliquot ſimul mixta in vaſe terreſtri ſuper ignem, perpetuo agitando comburuntur, donec brunnea evadant. Tunc ſuper lapidem cum adjecta aqua in pultem fere ſolidam rediguntur.

Fructus, prima et ſecunda ſua ætate, id eſt, Pollos et Herreli, ante præparationem decorticantur et inciduntur. In octo plerumque partes fructus primum inciduntur et ſingulum fruſtum ſeparatim decorticatur. Deinde ſingula pars inciditur in fruſta vel lamellas majores vel minores, craſſiores vel tenuiores pro uſu diverſo. Inciſio hæc adeo peragitur: cultri manubrium firmum tenetur inter hallucem et proximum pedis digitum, ita ut acies ſuperiora ſpectet et ſub medio poſito ligno apex elevetur, infra quem fructus inter ambas manus retentus inciditur. Incidentes tunc ſedere debent in ipſo ſolo vel pavimento

ſubſtrato

ſubſtrato tapete, vel in ſcamno humiliſſimo, ne altius ſedentes dorſi dolorem contrahant. Hoc modo conſcinduntur non modo fructus completi, et fructus abſque nucleis; ſed etiam nuclei, vel cum carne circumvolvente et tunicis, vel ab eis mundati. Hæc eſt methodus plerorumque Indianorum et quorundam Europæorum in Indiis degentium; ſed hæc quoque inciſio ſupra menſam cultro inſtitui poteſt. Conciſa omnia aqua purificantur ab arena et aliis adhærentibus immunditiis.

Nuclei a tunica carnoſa ſeparati a ceteris tunicis quoque mundantur. Exteriorem albam tunicam ſingulari modo ſeparant, ſcilicet nucleum perpendiculariter ſupra lapidem collocant et lapillo extremitatem ſupremam ſeu apicem pulſant, uſque dum diſrumpitur tunica et facile digitis ſeparari poteſt. Intimam brunneam tunicam cultro radendo eximunt.

Lac ſeu emulſio nucleorum Cocos, eodem, quo aliæ emulſiones modo, præparatur. Proprio primum inſtrumento nucleus raſpatur, aqua frigida additur, raſura inter digitos premitur et emulſio decolatur. Hoc *lac primum* vocant cum fortius ſit; ſi vero iterum raſuræ aqua ſuper infunditur et expreſſio fit, obtinetur *lac* ita dictum *ſecundum*, quod dilutius eſt.

Proportionem

Proportionem ingredientium circum circa indicavi, ſecundum quam ipſe vidi hæc fercula præparari; ſed illa tamen pro cujuſvis guſtu arbitraria eſt, ſic ut hoc vel illud ſecundum placitum augeri poſſit vel diminui.

XXXI. *A Second Paper concerning ſome Barometrical Meaſures in the Mines of the* Hartz. *By Mr.* John Andrew De Luc, *F. R. S.*

Read June 10, 1779.

THE Royal Society having been pleaſed to accept the communication which I had the honour to make to it two years ſince of certain barometrical experiments in the mines of the Hartz, I take the liberty of communicating to it ſome others of them, which I made the laſt year in one of the deepeſt of the mines in thoſe mountains, named the *Deep St. John*.

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Second Mémoire ſur des Meſures Barométriques dans les Mines du Hartz. *Par Monſ.* De Luc, *F. R. S.*

LA Société Royale ayant bien voulu agréer la communication que j'eus l'honneur de lui faire il y a deux ans de quelques expériences du barométre dans les mines du Hartz, je prends la liberté de lui en communiquer d'autres, que je fis l'année derniere dans une des mines les plus profondes de ces montagnes, nommée le *Profond St. Jean*.

It was on the 5th of July that I went down into it. I had been informed that I might not perhaps be able to arrive at the loweſt galleries becauſe of the foul air which had been there all the preceding days. Some miners therefore went before to examine how far we might go down without danger. Mr. USLAR, who had been ſo kind as to accompany me to the mines of Ramelſberg, was alſo pleaſed to be of this party, as well as Mr. FREDERIC, one of the officers of the mines, a gentleman well ſkilled in ſubterraneous geometry. And whilſt we were under ground Mr. MAYER, one of the clerks of the office of the mines, obſerved his barometer above ground every quarter of an hour.

It had been fine weather for ſome days, and that day it was particularly ſo; and Mr. MAYER perceived no variation

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Ce fut le 5° Juillet que j'y deſcendis. On me prévint que je ne pourrois peut-être pas arriver juſqu'aux galeries les plus baſſes à cauſe des mouffettes, dont il y avoit eu tous les jours précédens. Quelques mineurs prirent donc les devants pour examiner juſqu'a où nous pourrions deſcendre ſans danger. Monſ. DE USLAR, qui avoit eu la bonté de me conduire aux mines du Ramelſberg, voulut bien encore être de cette partie, de même que Mr. FREDERIC, l'un des officiers mineurs, très au fait de la géométrie ſouterraine. Et tandis que nous fûmes dans l'intérieur de la terre, Mr. MAYER, l'un des officiers du bureau des mines, obſerva ſon baromètre chaque quart d'heure à l'extérieur.

Il faiſoit fort beau depuis quelques jours, et ce jour là en particulier; et Mr. MAYER n'apperçut aucune variation dans ſon baromètre de 4 h. à 9 h. du ſoir:

riation in his barometer from four to nine o'clock at night, which, at that height, indicated the greateſt ſtability in the ſtate of the air in all other reſpects, excepting for the ordinary variations of the diurnal heat. Had it been on the Plain, the immobility of the barometer, in that part of the day, would have indicated that it had a tendency to deſcend. I ſhall not dwell on the cauſe of this difference, which reſults from that of the heights; I have explained it in my work on the atmoſphere.

It was $4\frac{3}{4}$ h. when I made, at the entrance of the mine, the firſt obſervation of the barometer and of the thermometer which accompanies it; and at the bottom of the firſt ladder, that of the detached thermometer. Continuing to deſcend we arrived at $5\frac{1}{2}$ h. towards the middle of the depth of the mine, near the beginning of a gallery

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ce qui, à cette hauteur, marquoit la plus grande ſtabilité dans l'état de l'air, à tout autre égard que pour les variations ordinaires de la chaleur diurne. A la Plaine, l'immobilité du Barométre dans cette partie du jour, eût marqué qu'il tendoit à deſcendre. Je ne m'arrêterai pas à la cauſe de cette différence, qui reſulte de celle des hauteurs; je l'ai expliquée dans mon ouvrage ſur l'atmoſphére.

Il étoit $4\frac{3}{4}$ h. lorſque je fis à l'entrée de la mine la premiére obſervation du barométre et du thermométre qui l'accompagne; et au bas de la premiére échelle celle du thermométre détaché. Continuant à deſcendre, nous arrivâmes à $5\frac{1}{2}$ h. vers le milieu de la profondeur de la mine, auprès du commencement d'une

a gallery named the George, where I made an obſervation at $5\frac{3}{4}$ h.

The miners who had gone before us, having come back from the bottom of the mine, reported that the vapours were diſſipated, and that the air was every where good. So we continued to deſcend to the place where, for want of a ſufficiently deep draining gallery, the water is obliged to be left, although by ſome extraordinary efforts they had formerly ſunk the ſhaft ten or twelve fathoms deeper. This point is at the level of the eleventh gallery. I obſerved there at $7\frac{1}{2}$ h. The air was perfectly wholeſome, only a little warmer than at the top of the ſhaft.

In aſcending again I repeated the obſervation at $8\frac{1}{2}$ h. near the George gallery; the barometer ſtood then one eighth of a line higher than the firſt time. On returning

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galerie nommée George, où je fis une obſervation à $5\frac{3}{4}$ h.

Les mineurs qui nous avoient précédés, étant alors de retour du fond de la mine, nous rapportérent que les mouffettes étoient diſſipées, et que l'air étoit bon partout. Ainſi nous continuâmes à deſcendre juſqu'au point où, manque d'une galerie d'écoulement aſſez profonde, on eſt obligé de laiſſer l'eau; quoique, par des efforts, on aît approfondi autre fois le puits de 10 à 12 toiſes de plus. Ce point eſt à niveau de l'onziéme galerie. J'y fis l'obſervation à $7\frac{1}{2}$ h. L'air y étoit parfaitement ſain; ſeulement il étoit un peu plus chaud que dans le haut du puits.

En remontant, je réiterai l'obſervation à $8\frac{1}{2}$ h. auprès de la galerie George; le baromètre s'y tint plus haut d' $\frac{1}{8}$ de ligne que la première fois. De retour à

9 heure

ing to the top of the mine at nine o'clock, I observed there again, and found the barometer one sixteenth of a line higher than when I had entered.

Combining these observations, which are set down at the end of this paper, I found that the George gallery was 127·15 lachters, or Hartz fathoms, below the entrance of the mine; and that the whole depth was 215·86 lachters, which makes 801 English feet for the first depth, and 1359 for the latter.

Mr. USLAR, having taken a memorandum of the places where I had made my observations, afterwards gave me the correspondent geometrical measures. The depth of the George gallery he found to be 127·87 lachters, that is, only four feet more than what I had found by the barometer; but I was much surprised to see that, in the whole

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9 heures au haut de la mine, j'y observai encore, et je trouvai le baromètre plus haut d' $\frac{1}{16}$ de ligne que lorsque j'étois entré.

Combinant ces observations, dont les détails sont à la fin de ce mémoire, je trouvai que la galerie George étoit abaissée de 127,15 lachters, ou toises du Hartz, au dessous de l'entrée de la mine; et que la totalité de la profondeur étoit de 215,86 lachters. Ce qui fait 801 pieds Anglois pour la prémière profondeur, et 1359 pour la derniere.

Mr. DE USLAR ayant pris note des lieux où j'avois fait mes observations, me donna ensuite les mesures géométriques correspondentes. La profondeur de la galerie George se trouvoit être de 127,17 lachters c'est à dire plus grande seulement de 4 pieds que celle que j'avois trouvée par le baromètre: mais je fus bien

whole depth, the geometric meaſure exceeded mine by 110 feet.

At firſt I ſuſpected an error in my obſervation, and I reſolved to go down into the mine again. But afterwards, from a compariſon of the ſucceſſive obſervations, in which the particular conſtruction of the ſcale of my barometer would have ſhewn the error, I ſaw clearly that my obſervation was well made. I ſhall not in this place explain the nature of that examination, for fear of being too long; but it is eaſily deduced from the conſtruction of that ſcale.

I had no longer reaſon to think, that the barometric meaſure could in itſelf differ ſo much from the geometric meaſure, ſince they agreed ſo well in the middle of the ſhaft. There remained nothing to be ſuſpected but the geo-

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ſurpris de voir que ſur la profondeur totale, la meſure géométrique excédoit la mienne de 110 pieds.

Au premier moment je ſoupçonnai une erreur dans mon obſervation, et je me réſolus de retourner dans la mine. Mais enſuite, par une comparaiſon des obſervations ſucceſſives, dans laquelle la conſtruction particuliere de l'echelle de mon barométre eût fait appercevoir cette erreur, je vis ſans aucun doute que mon obſervation étoit bien notée. Je n'expliquerai pas ici la nature de cet examen, de peur d'être trop long; mais on le conclura aiſement de la conſtruction de cette echelle.

Je n'avois point de raiſon non plus de croire que la meſure barométrique pût en elle même s'écarter autant de la meſure géométrique; puis qu'elles s'accordoient ſi bien dans le milieu du puits. Il ne reſtoit donc à ſuſpecter que la note qui

geometric meaſure which had been given me, and I requeſted Baron DE REDEN to cauſe it to be examined. The perſon whom he charged with that care was his nephew, who is very ſkilful in every thing concerning the mines. He examined with Mr. RAUSCH, the chief geometer, the meaſure which had been given me; and they found, that in writing the depths of the differents parts of the mine, the ſum of which ought to give the whole depth, one of theſe parts had been twice put down, ſo that the true depth was only 215½ lachters: it became then two feet leſs than that which reſulted from my obſervations.

Thus the barometric meaſurement gave four feet leſs than the geometrical meaſure for the former of the depths, and two feet more for the latter.

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qui m'avoit été donnée de la meſure géométrique; et je priai M. le Baron DE REDEN de la faire examiner. La perſonne qu'il chargea de ce ſoin, fut ſon neveu, qui eſt très éclairé dans tout ce qui concerne les mines. Il vérifia donc cette note avec Mr. RAUSCH le géométre en chef; et ils trouvérent, qu'en prenant en détail les profondeurs des différentes parties de la mine, dont la ſomme devoit donner la profondeur totale, on avoit poſé deux fois une de ces parties; deſorte que la vraie profondeur n'étoit que 215½ lachters. Elle étoit donc alors moindre de 2 pieds, que celle qui reſultoit de mes obſervations,

Ainſi la meſure barométrique avoit donné 4 pieds de moins que la meſure géométrique pour la premiére des profondeurs, et 2 pieds de plus pour la derniere.

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I was at firſt as much ſurprized as ſatisfied with this exactneſs, which I did not expect notwithſtanding my former experience; and I ſhould not have been ſurprized to find two or three fathoms difference either way. But, reflecting on it afterwards, I perceived that there ought to be more certainty in the barometric meaſure for the depth of mines, than for the height of mountains; and that for the two following reaſons.

Firſt, that in making theſe obſervations along the ſhafts of mines we go but little from the ſame vertical column, and by that we know with certainty the weight which compreſſes the part which we meaſure: whereas in mountains, even the ſteepeſt, the obſervations are made in columns of air which are far enough from one another to be liable to ſenſible differences on that account; and

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Je fus d'abord auſſi ſurpris que content de cette exactitude, à laquelle je ne m'attendois point malgré mes précédentes expériences; et je n'aurois pas été étonné de trouver 2 au 3 toiſes d'écart dans l'un ou l'autre ſens. Cependant, y réfléchiſſant enſuite, j'ai compris qu'il doit y avoir, plus de ſureté dans la meſure barométrique pour la profondeur des mines, que pour la hauteur des montagnes; et cela par les deux conſidérations ſuivantes.

La premiére, qu'en faiſant ces obſervations le long des puits des mines, on s'écarte peu d'une même colonne verticale; et l'on connoit ainſi ſurement le poids qui comprime la partie qu'on meſure. Aulieu que dans les montagnes, même les plus rapides, les obſervations ſe font dans des colonnes d'air aſſez éloignées l'une de l'autre, pour qu'il puiſſe y avoir des différences ſenſibles à cet égard; et

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and that if, for example, the higher barometer were moved horizontally as far as the column which reſts upon the ſtation of the lower one, it would not ſtand at the ſame point as when it was on the mountain; which, however, is ſuppoſed in the formula.

The other circumſtance reſpects the homogeneity of the air. In the open air a thouſand lateral cauſes may introduce into the portion of the column which we meaſure ſome layers of air different from thoſe of the two extremities, were it only in point of heat. And in that reſpect the correſpondent obſervations neareſt the foot of the mountain are not always the moſt certain, becauſe of the reflexions of the ſun, and even of the winds which carry the vapours; and the reſult muſt be that we very often find the heights too ſmall. I ſhall not ſtop to prove

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que par exemple, ſi le barométre ſupérieur étoit avancé horizontalement juſqu'à la colonne qui pèſe ſur la ſtation inférieure, il ne ſe tint pas au même point que ſur la montagne; ce qui néantmoins eſt ſuppoſé par la formule.

L'autre circonſtance regarde l'homogénéité de l'air. En plein air, mille cauſes latérales peuvent introduire dans la portion de colonne qu'on meſure, des couches d'air différentes de celles des deux éxtrémités; ne fût-ce que différemment chaudes. Et à cet égard les obſervations correſpondentes les plus près du pied des montagnes ne ſont pas toûjours les plus ſûres à cauſe des réflexions du Soleil, et de celles des vents qui charient les vapeurs: et il doit en réſulter très ſouvent qu'on trouve les hauteurs trop petites. Je ne m'arrêterai pas à le mon-

prove it. In mines, on the contrary, the air being incloſed as in a canal, where it is always in motion, it continually intermixes and becomes more homogeneous: it contracts there a temperature which is more uniform or more gradually different, ſo that we can more certainly fix the mean ſtate of it.

On theſe two accounts, therefore, barometric meaſures ought to be more regular in moſt mines than on the ſides of mountains; becauſe the obſervations are made nearly at the extremities of one and the ſame column, and becauſe the whole column is more nearly ſimilar to its extremities; conſequently we are ſurer that the particular caſe is not an exception to the rules. Now it has happened, that in all the obſervations which I have made in the Hartz mines, my rules, drawn from a mean among my obſervations in open

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trer. Dans les mines au contraire, l'air étant renfermé comme dans un canal, où il eſt toûjours en mouvement, ſe mêle ſans ceſſe et ſe rend plus homogène: ſurtout il y contracte une température plus uniforme ou plus graduellement différente; enſorte qu'on peut plus ſurement en fixer le terme moyen.

Les meſures barométriques doivent donc, par ces deux conſidérations, être plus régulières dans la plûpart des mines, que ſur les pentes des montagnes; c'eſt à dire parce que les obſervations ſont faites à peu prés aux extrémités de la même colonne, et que la totalité de la colonne a des rapports plus reguliers avec ſes extrémités. Ainſi l'on eſt plus ſûr que le cas n'eſt pas une exception aux régles. Or il ſe trouve en même tems, que dans toutes les obſervations, que j'ai faites dans les mines du Hartz, mes régles, tirées du milieu entre mes obſervations en plein

open air, have agreed with the geometric meaſures, which ſeems to indicate that the uſual ſtate of the air of the mines is a mean among the different ſtates of the exterior air as found in the whole of my obſervations; the almoſt conſtant ſtate of the heat in the mines may be the principal cauſe of it.

As to the geometric meaſures themſelves, I might here give ſome very intereſting proofs of their exactneſs by the deſcription of the aſtoniſhing works which are undertaken in conſequence of them; but I ſhould be afraid of being too tedious. And, beſides, we may depend on the miners for the exactneſs of thoſe meaſures, they being more intereſted in them than we are. I ſhall, therefore, conclude this paper with ſome general remarks on barometrical meaſurement.

So

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plein air, s'y ſont accordées avec les meſures géomètriques; ce qui paroit indiquer, que l'état habituel de l'air des mines, eſt moyen entre les divers états où il s'eſt trouvé dans la totalité de mes expériences; et l'état preſque conſtant des mines quant à la chaleur, pourroit bien en être la principale cauſe.

Quant aux meſures géomètriques elles mêmes, je pourrois donner ici des preuves tres intéreſſantes de leur exactitude, par le tableau des ouvrages étonnans qu'on entreprend d'après elles; mais je craindrois d'être trop long. Et d'ailleurs nous pouvons nous en rapporter aux mineurs ſur l'exactitude de ces meſures; ils y ont un plus grand interêt que nous. Je conclurai donc ce memoire par quelques remarques générales ſur la meſure barométrique.

So long as in this meaſurement we ſhall conſider as given only the differences in the weight and heat of the air at the places of obſervation, we ſhall be ſubject to errors; becauſe there are many other cauſes of modification in the air: and all the exactneſs to which we can pretend will be to determine a formula which preſerves a mean among the poſſible variations.

This is what I have propoſed to do in my own formula; and it ſeems to anſwer this end. In the different trials which have been made of it, it has ſometimes given the heights too great, at other times too little, without diſtinction of climate. Thus, for example, the experiments at Spitzbergen by Lord MULGRAVE, and at the Pike of Teneriffe by Mr. DE BORDA, one of the French academicians, gave the heights too great; thoſe of Col.

Tant qu'on n'aura pour données dans cette meſure, que les differences de poids et de chaleur de l'air aux lieux des obſervations, on ſera ſujet à des erreurs; parce qu'il y a bien d'autres cauſes qui modifient l'air: et toute l'exactitude à laquelle on pourra prétendre, ſera de déterminer une formule qui garde le milieu entre les écarts poſſibles.

C'eſt là ce que je me ſuis propoſé dans la mienne, et elle paroît répondre à ce but. Dans les diverſes épreuves qui en ont été faites, elle a donné quelquefois les hauteurs trop grandes, d'autres fois trop petites, ſans diſtinction de climat. Ainſi par exemple; éprouvée au Spitzberg par mylord MULGRAVE, et au Pic de Ténériffe par M. DE BORDA l'un des Académiciens François, elle a donné les hauteurs trop grandes; et dans les obſervations de M. le Col. ROY et de M. le Chev.

Col. ROY and Sir GEORGE SHUCKBURGH *, made in mean latitudes, and partly in the places where I myſelf had obſerved, gave them too little.

Theſe differences, do not ſeem to depend on the climate, and indeed I have frequently obſerved them to happen in the ſame places. Thus, for example, my obſervation on the Glaciere de Buet, cited by Sir GEORGE SHUCKBURGH, gives the height of that mountain a little leſs than the geometrical meaſure; but Mr. DE SAUSSURE having repeated the barometrical obſervation, it agreed with that meaſure by the ſame formula; and Mr. MARC PICTET, by a third obſervation, found the height a little too great. In theſe three obſervations, the particulars of which I omit, the correſponding point was Geneva, diſtant about ten or twelve leagues.

At

Chev. SCHUCKBURGH, faites dans des latitudes moyennes, et en partie dans les lieux où j'ai moi même obſervé, elle les a donné, trop petites.

Ces différences ne paroiſſent donc pas tenir aux climats; et en effet je les ai obſervées frequemment dans les mêmes lieux. Ainſi par exemple, mon obſervation ſur la Glaciere de Buet, citée par M. le Chev. SCHUCKBURGH *, donne la hauteur de cette montagne un peu moindre que la meſure géomètrique: mais M. DE SAUSSURE ayant répété l'obſervation barométrique, ſe trouva d'accord avec cette meſure, par la même formule; et M. MARC PICTET, dans une troiſième obſérvation, trouva la hauteur un peu trop grande. Dans ces trois obſervations, dont je ne rapporte pas les détails, le point correſpondant étoit Géneve, diſtant de 10 à 12 lieues.

* Phil. Tranſ. 1777.

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At that diſtance there are doubtleſs ſome cauſes of variations which are irremediable; ſince the formula ſuppoſes that the obſervations are made in the ſame column of air. It is therefore only in the caſes in which that ſuppoſition approaches near the truth that we can hope to perfect the rule. But this can only be by introducing new conditions into it; that is, other modifications of the air, of which we have not as yet taken any account.

In meditating on the cauſes of the diverſity of reſults in experiments, it has always appeared to me, that the differences of the effects of heat on the air, according to the different ſtates of it, was the principal; that is, that the air not being always of the ſame nature, heat, that grand cauſe, whoſe effects we ought principally to determine, does not always act equally. Beſides the particular experiments

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A cette diſtance ſans doute il y a des cauſes d'écarts qui ſont irrémédiables; puiſque la formule ſuppoſe que les obſervations ſont faites dans une même colonne d'air. Ce n'eſt donc que pour les cas où cette ſuppoſition approche de la verité, qu'on peut eſpérer de perfectionner la régle. Mais ce ne ſera qu'en y introduiſant de nouvelles conditions; c'eſt à dire d'autres modifications de l'air dont on n'a pas tenu compte juſqu' ici.

En étudiant les cauſes de la diverſité des reſultats dans les expériences, il m'a toujours paru, que la différence des effets de la chaleur ſur l'air, ſuivant qu'il eſt lui même conſtitué, étoit la principale: c'eſt à dire, que l'air n'étant pas toûjours de même nature, la chaleur, cette grande cauſe dont il faut principalement déterminer les effets, n'y agit pas toujour également. Outre les expériences particuliéres,

periments which prove it, we can attribute to theſe differences only thoſe of the reſults of the reſearches of ſome philoſophers concerning the dilatations of the air by heat, applied to various phyſical uſes.

In a paper lately read at the Royal Society, on the ſubject of refractions, I analyſed and compared different formulæ of this kind, given by philoſophers on whom we can depend. The reſult of that examination was that, ſuppoſing the volume of air = 1000 when the Engliſh thermometer of FAHRENHEIT is at 32°, if the heat of the air be increaſed 22,8 degrees of this thermometer, its volume will be,

According

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particulières, qui le prouvent, on ne peut attribuer qu'à ces différences, celles des réſultats des recherches de divers phyſiciens ſur les dilatations de l'air par la chaleur, appliquées à divers uſages phyſiques.

C'eſt à l'occaſion des réfractions, que, dans un mémoire lu il y a peu de tems à la Société Royale, j'ai analyſé et comparé différentes formules de cette eſpéce, données par des phyſiciens, ſur qui l'on peut compter. Il eſt réſulté de cet examen, qu'en ſuppoſant le volume de l'air = 1000 lorſque le thermométre Anglois de FAHRENHEIT eſt à 32°, ſi la chaleur de l'air augmente de 22,8 degrès de ce thermométre, ſon volume ſera:

Suivant

According to Mr. l'Abbé DE LA CAILLE,		1040
- - -	Mr. Profeſſor MAYER,	1046
- - -	Mr. BONNE, - -	1047,7
- - -	Sir GEORGE SHUCKBURGH,	1050,5
- - -	Dr. BRADLEY, -	1054,4

Here then are great differences in this point only, namely, the effect of heat on the denſity of the air; differences which muſt have a viſible effect in all meteorological phenomena. Doubtleſs they proceed, in a great meaſure, from the different degrees of dryneſs of the air, which we can no longer doubt of, ſince the intereſting experiments of Col. ROY with the manometer *. This is the ſame cauſe to which I imputed the greateſt variations in my experiments in open air, and which obliged me to conclude

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Suivant M. l'Abbé DE LA CAILLE,	1040
Suivant M. le Prof. MAYER,	1046
Suivant M. BONNE,	1047,7
Suivant M. le Chev. SHUCKBURGH,	1050,5
Suivant M. le Dr. BRADLEY,	1054 4

Voila donc de bien grandes différences ſur ce ſeul point, l'effet de la chaleur ſur la denſité de l'air; différences qui doivent agir dans tous les phénoménes météorologiques. Elles procédent ſans doute en plus grande partie des différens degrés de ſéchereſſe de l'air; on ne peut plus en douter depuis les intéreſſantes expériences de M. le Col. ROY avec le manomètre *. C'eſt cette même cauſe que j'avois ſoupconnée de produire le plus d'ecarts dans mes expériences en plein air, et qui m'obligea à conclure ma correction pour les effets de la chaleur ſur

* Phil. Tranſ. 1778.

conclude my correction for the effects of heat upon the air from a mean among my numerous obſervations. Now this mean, reduced to the ſame term of compariſon as above, is 1047, which is alſo a mean among thoſe different reſults.

Theſe differences therefore ſhew us, that atmoſpherical air is not a ſubſtance that is homogeneous and conſtantly the ſame; and that next to the care of making as many obſervations as poſſible in the maſs of air itſelf which it concerns us to know, the only means of determining more exactly its various influences is to multiply our meteorological inſtruments.

Barometrical

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l'air, par un milieu entre mes nombreuſes obſervations. Or ce milieu, reduit au même terme de comparaiſon ci deſſus, eſt 1047, qui ſe trouve être auſſi le milieu entre ces divers réſultats.

Ces différences nous montrent donc; que l'air atmoſphérique n'eſt pas une ſubſtance homogéne et conſtamment la même; et qu' après le ſoin de faire autant qu'il eſt poſſible, les obſervations dans la maſſe même de l'air qu'il nous importe de connoître, le ſeul moyen de déterminer plus exactement ſes diverſes influences, eſt de multiplier nos inſtrumens de météorologie.

Barometrical Obſervations in the Deep St. John, July *the* 5*th*, 1778.

	Time.		Bar. corrected by its therm.	Therm. in the air.
	h	′	Inches.	
At the ent. of the mine,	4	50 night	26.6224	− 5
	2	5 night	26.6276	− $7\frac{1}{2}$
The mean,		-	26.625	− $6\frac{1}{4}$
At the George Gallery,	5	50 night	27.4167	−17
	8	15 night	27.4271	−17
The mean,		-	27.4219	−17
At the level of the 11th G.	7	30 night	27.9792	− 8

Calculation

Obſervations barométriques au Profond St. Jean, *le* 5e Juillet, 1778.

			Bar. corrigé par ſon therm.	Therm. dans l'air.
	h	′	P. L.	
A l'entrée de la mine,	4	50 ſoir	26.7 $\frac{15}{32}$	− 5
	9	5 ſoir	26.7 $\frac{17}{32}$	− $7\frac{1}{2}$
		Terme moyen,	26.7 $\frac{8}{16}$	− $6\frac{1}{4}$
A la galerie George,	5	50 ſoir	27.5	−17
	8	15 ſoir	27.5 $\frac{2}{16}$	−17
		Terme moyen,	27.5 $\frac{1}{16}$	−17
Au niveau de l'onzième gal.	7	30 ſoir	27.11 $\frac{12}{16}$	− 8

Calcul

Calculation of the Depth of the George Gallery.

Barometer at the gallery,	27.4219 = 5265 (192ds of an inch)	Log.	37213984
At the entr. of the mine,	26.6250 = 5112		37085908
		Difference	128076
Heat of the air at the gallery,	− 17		
At the entrance of the mine,	− $6\frac{1}{4}$		
	$-23\frac{1}{4} = \frac{23\frac{1}{4}}{1000}$ to deduct		2977
	1000dths of a French fathom,		125099
	$\frac{1}{61}$ to add, − −		2051
		Lachters	127.150

Calculation

Calcul de la profondeur de la Galerie George.

Bar. à la Gal. −	27.5.1 = 5265 (16^{mes} de lig.)	Log.	37213984
A l'entrée de la mine,	26.7.8 = 5112		37085908
		Difference	128076
Chaleur de l'air à la Gal −	− 17		
A la entrée de la mine, − −	− $6\frac{1}{4}$		
	$-23\frac{1}{4} = \frac{23\frac{1}{4}}{1000}$ à deduire		2977
	1000^{me} de toise de France, −		125099
	$\frac{1}{61}$ à ajouter, − −		2051
		Lachters	127,150

Calculation of the Depth of the 11*th gallery.*

Barometer at the gallery,	27.9792 = 5372	Log.	37301360
At the entrance of the mines,	26.6250 = 5112		37085908
		Difference	215452
Heat of the air at the gallery,	– – 8		
At the entrance of the mine,	– – $6\frac{1}{4}$		
	$-14\frac{1}{4} = \frac{14\frac{1}{4}}{1000}$	to deduct	3070
	1000dths of a French fathom,		212382
	$\frac{1}{61}$ to add, – –		3482
		Lachters	215.864

Calcul de la profondeur de l'onziéme galerie.

Bar. à la galerie, –	$27.11\frac{12}{16} = 5372$	Log.	37301360
A l'entrée de la mine –	$26.\ 7\frac{8}{16} = 5112$		37085908
		Difference	215452
Chaleur de l'air à la gal. –	– 8		
A l'entrée de la mine, –	– $6\frac{1}{4}$		
	$-14\frac{1}{4} = \frac{14\frac{1}{4}}{1000}$	à deduire	3070
	1000^{me} de toise de France,		212382
	$\frac{1}{61}$ à ajouter, – –		3482
		Lachters	215,864

XXXII. *On the Preceſſion of the Equinoxes produced by the Sun's Attraction.* *By the Rev. Mr.* Iſaac Milner, *M. A. and Fellow of* Queen's College, Cambridge; *communicated by the Rev. Dr.* Shepherd, *F. R. S.*

Read June 24, 1779.

IF the actions of the Sun and Moon upon the different parts of the earth were equal; or if the earth itſelf were perfectly ſpherical, and of an uniform denſity from the center to the ſurface; in either caſe the attractions of thoſe remote bodies would have no effect on the poſition of the terreſtrial equator, and the equinoctial points would conſtantly be the ſame in the heavens. But it was impoſſible to give the earth a rotatory motion round an axis without giving at the ſame time a centrifugal force to its parts. This force is greateſt at the equator, and is in a contrary direction to that of gravity; on either ſide of the equator the force is leſs; and, moreover, only part of its effects is oppoſed to that of gravity. It is uſual in determining the figure of the earth to conſider the whole maſs as in a ſtate of fluidity, and the different columns as ſuſtaining one another at the center. If the

earth

earth be conſidered as a hard body, firmly cohering in its parts by ſome other force beſides that of gravity, it does not ſeem neceſſary that the different columns ſhould be ſuppoſed to ſuſtain each other at the center, though in both caſes the direction of gravity muſt at every point of the ſurface be perpendicular to the tangent of the figure. But we know, that there is a conſiderable quantity of water upon the ſurface of the earth; and, therefore, if the equatorial regions were not higher than the polar, they certainly would be overflowed by the Ocean, which is contrary to experience; and for this reaſon the proportion of the diameters of the earth, determined upon the falſe ſuppoſition of an entire fluidity, cannot differ much from the truth.

§ 2. But the preceſſion of the equinoxes, which depends upon the unequal actions of the Sun and Moon on the protuberant parts of the earth at the equator, will not be the ſame in theſe different hypotheſes; at leaſt we can never be certain that it will be ſo until we have computed their effects, and the computation itſelf muſt proceed on different principles. Suppoſe the earth to be fluid under the form of an oblate ſpheroid; or, what is more ſimple, ſuppoſe the region of the equator to be ſurrounded with a ring of fluid matter, and the unequal action of the Sun will diſturb the figure of the ring, and

communicate

communicate a motion to its parts. Suppoſe we knew the preciſe diſturbing force of the Sun upon any one particle of this ring according to its ſituation; in that caſe we could eaſily find the velocity which would be communicated to ſuch a particle in any given time; but the mutual actions of the fluid particles upon each other could never be exactly eſtimated, much leſs their effects in endeavouring to turn the whole earth round its center. However, it is eaſy to ſee, that in the caſe of a hard ring of matter cohering cloſe with the ſurface of the earth at the equator, both the law by which the particles act on each other, and on the whole maſs of the earth, will be widely different from the caſe of fluidity, and the effects much greater in altering the poſition of the axis of rotation.

To explain this by an eaſy example (fig. 1,) let A, B, and C, repreſent three ſmall bodies in the ſame horizontal line AE. Suppoſe A to deſcend by any accelerating force as gravity; B to deſcend by the ſame force, a leſs or a greater; and C not to be acted upon at all: in every one of theſe caſes the bodies A and B will deſcend with their reſpective velocities, and the body C will preſerve its ſituation. If A and B are ſmall particles of fluid of any form, and C a hard one, and if the particle A be placed

in contact with B, have its center of gravity a little above the center of gravity of B, and is acted upon by the greater accelerating force; in this case we may conceive how the action of A may disturb the motion of B, and in the same way how the hard particle C may receive a small motion from the actions of A and B. Then this motion must be extremely little compared with the whole motions of A or B, and still a great deal less if C be strongly connected with a string of hard particles along the line CE, so that C cannot be moved without the whole line CE turning round the immoveable center E. Now if A, B, and C, be supposed hard particles firmly connected to the lever AE, then it is plain that the velocity of C, whatever it is, must be in proportion to that of A and B as their respective distances from E the center of motion, and this, whatever the impulsive forces are with which A and B are urged in their respective directions.

The body C being still supposed void of gravity, let the bodies A and B be urged by forces perpendicular to AE in any small equal times through the unequal spaces s and s', and let the magnitudes of the bodies be represented by A, B, and C respectively. Then the space through which A is actually urged in that time will easily appear from mechanics to be represented by

$\frac{A \times AE \times s + B \times BE \times s' \times AE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$, and the ſpace deſcribed by C is

$\frac{A \times AE \times s + B \times BE \times s' \times CE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$. See article the 13th.

§ 3. The preceding article being well underſtood, whatever doubts may remain concerning the motion of a ring of matter conſidered as detached from the earth, we may be certain that the motion of the nodes of the equator can never be the ſame, whether we ſuppoſe the ring at the equator to be fluid and to reſt upon the ſurface of the earth, partaking of the diurnal motion, or whether we ſuppoſe it hard and compact, and by its coheſion communicating a proportional degree of motion to the different parts of the earth. In fact, the problem of the preceſſion of the equinoxes, which has hitherto been conſidered as extremely difficult, and in its ſolution drawn out by authors to an immeaſurable length, requires no principles but the received doctrine of motion, and the application of the lever, which have been made uſe of in the laſt article. In that article we ſuppoſed the bodies A and B to be impelled by different forces in parallel lines, and we eſtimated the real ſpace, which either A or C in any ſmall time would deſcribe in conſequence of thoſe impulſive forces and their mutual connection by an inflexible lever. Now this is preciſely what is re-

quired to be done in the caſe of the Sun's unequal action on the protuberant parts of the equator. The exceſſes or defects of that unequal action are to be conſidered as forces applied to thoſe parts, which would move them according to the different circumſtances through unequal ſpaces proportional to the forces in equal times of action, provided the particles were at liberty to move freely in the directions in which they are urged; and, laſtly, the real ſpace muſt be computed through which a particle moves at ſome known diſtance from the center of the earth in conſequence of theſe various forces. This whole proceſs will not differ from the eaſy example already deſcribed, except in the length of the calculation, and the proper management of the doctrine of fluxions; and it ſeems adviſable in difficult ſubjects always to begin with ſimple inſtances before we proceed to thoſe which are more complex, and to diſtinguiſh the algebraical operations from the principles upon which they are founded.

§ 4. In order to determine how much any particle of the earth is affected by the unequal action of the Sun (fig. 2.), let CADB repreſent the earth, S the Sun at a great diſtance, and CD a plane perpendicular to the line ST joining the centers of the Sun and earth. If SK or ST repreſent the accelerating force of the Sun on a particle at the earth's center, and SL be taken to SK in the duplicate

ratio

ratio of ST to SP; SL will represent the attraction on any particle P, and by the resolution of motion T*m* or PL will represent the perturbing force of the Sun on the same particle. By the construction $SL : SK :: SK^2 : SP^2$, and by division $KL : SK :: SK^2 - SP^2 : SP^2 :: \overline{SK + SP} \times PK : SP^2$ and PL or T*m* is nearly equal to 3PK, and as 3PK is to SK or ST, so is the space described by P in any small time in the direction PK, to the space described in the same time by the center of the earth in consequence of the Sun's attraction. This last space is equal to $\frac{\dot{z}^2}{2ST}$ where $\dot{z}$ represents the arc described by the earth's center during any small motion in its orbit, and the former is equal to $\frac{3PK \times \dot{z}^2}{2ST^2}$. This is the space which would be described by P in the direction PK if the particle was at liberty to move freely. Let us at present suppose that no other particle is disturbed by the Sun's attraction except this one, and then proceed to enquire into the effects of this disturbance when P by its cohesion communicates a motion to the different parts of the earth, which is farther constrained to turn round an axis T, the common intersection of the plane CD and the terrestrial equator. From the laws by which motion is communicated, and the property of the lever, it easily appears, as in the second article, that the space through which any particle of the

earth's equator is impelled at the greateſt diſtance from the axis T, is to $\frac{3PK\dot{\varepsilon}^2}{2ST^2}$, the ſpace which would be deſcribed in the ſame time by any particle at liberty, the magnitude of which is repreſented by P, as $P \times KT \times$ the radius of the equator to the ſum of all the particles of the earth multiplied into the ſquares of their reſpective diſtances from the ſaid axis.

To compute this ſum in the eaſieſt way, and by an approximation, which is quite ſufficient when the polar and equatorial diameters differ little from one another; let DPE (fig. 3.) be a ſphere whoſe radius is unity, divided into an infinite number of thin cylindrical ſurfaces, whoſe baſes are the circles NAQ; it is obvious, that all the particles in any one of theſe ſurfaces are at the ſame diſtance $CA = x$ from the axis of motion perpendicular to the plane of the circle NAQ. Call AP, y, and A, the area of the circle DPE and the fluent of $4Ax^3\dot{x}y$, or of $-4Ax^2y^2\dot{y}$, becauſe $x\dot{x} = -y\dot{y}$ gives the ſum of all the particles in the ſphere multiplied into the ſquares of the reſpective diſtances from the axis. This fluent corrected is equal to $\frac{8A}{15}$, and muſt now be diminiſhed in the ratio of 1 to $1-2p$, if we ſuppoſe the earth to be an oblate ſpheroid whoſe equatorial diameter is to the polar as 1 to $1-p$;

and, laſtly, the ſpace deſcribed by a particle at the greateſt diſtance from the axis is equal to $\frac{45P \times PK \times KT \times \dot{z}^2}{16A \times ST^2 \times 1-2p}$.

§ 5. In fig. 4. let PIA*p*DK repreſent the earth orthographically projected on the plane of the ſolſtitial colure, P, *p*, the poles, IK a leſſer circle parallel to the equator, and P*ape* a ſphere deſcribed with the polar radius PT: then, ſince the particles without the globe only are concerned in changing the poſition of the axis of rotation, let L repreſent ſuch a particle ſituated in the circumference of the circle IK, and by the preceding article its effect will be $\frac{45L \times LM \times MT \times \dot{z}^2}{16A \times ST^2 \times 1-2p}$, and by the ſame way of reaſoning, when two equal particles L, *l*, are ſuppoſed to be diſturbed by the Sun's attraction, the ſpace deſcribed by that point of the equator, which is at the greateſt diſtance from the axis of rotation or the common interſection of the plane CD and the equator A, will be equal to $\frac{45\dot{z} \times \overline{L \times LM \times MT + l \times lm \times mT}}{16ST^2 \times A \times 1-2p}$, and the ſame argument holds for every other particle without the ſphere.

The ſum of all the $L \times LM \times MT$ + &c. muſt now be found; and for this purpoſe Sir ISAAC NEWTON's conſtruction is, perhaps, as convenient as any that has hitherto appeared. In the ſame figure N*n* is parallel, and *xy* perpendicular, to CD; take $Lx = xl$, and let *m*, *n*, repreſent

ſent the ſine and coſine of the angle CTP to the radius unity. It is eaſy to prove in his way that $\overline{\text{L}\times\text{LM}\times\text{MT}+l\times lm\times m\text{T}}$ is equal to $2\text{L}\times m\times n\times\overline{\text{L}x^2-\text{T}x^2}$, and the fluent of $\text{L}x^2$ multiplied into the fluxion of the circular arc $\text{L}x$ is eaſily found in the following manner, without having recourſe to tables of fluents, or the methods of continuation.

From a known analogy the fluxion of the arc $\text{L}x$ is to the fluxion of its verſed ſine as the radius $\text{I}x$ of the ſame circle to $\text{L}x$ the right ſine. $\text{L}x$ multiplied into the fluxion of the verſed ſine is the fluxion of the area of the ſemi-circle $\text{L}l$, and calling $\text{I}x, y$, the fluent of $\text{L}x^2$ multiplied into the fluxion of the arc $\text{L}x$ is evidently equal to $\frac{y^3\text{A}}{2}$, where A ſtill repreſents the area of a circle whoſe radius is unity: $\text{A}y$ is equal to the ſemi-circumference IK and $\text{A}y\times\text{T}x^2$ is equal to the fluent of $\text{T}x^2$ multiplied into the ſame fluxion, and calling $\text{T}x$, v, and ſubſtituting for $\text{I}i$ its equal py, the ſum of all the $\text{L}\times\text{LM}\times\text{MT}+\&\text{c}.$ in the annulus $\text{I}i$ is equal to $mnp\,\text{A}\times\overline{y^4-2y^2v^2}$. This laſt quantity multiplied into the fluxion of v, and the fluent taken by the common method when v is equal to TP or unity nearly, comes out $\frac{4p\text{A}mn}{15}$ and twice this quantity gives the ſum of all the $\text{L}\times\text{LM}\times\text{MT}$, without the whole ſphere $\text{P}ap$, and therefore the ſpace deſcribed by a particle of the equa-

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tor in the circle of the Sun's declination while the center of the earth is carried through the ſpace $\dot{z}$ of its orbit is equal to $\frac{3pmn\dot{z}^2}{2sT^2 \times 1-2p}$, and may be ſuppoſed to be equal to $\frac{3pm\dot{z}^2}{2sT^2}$, the alteration produced by the correction in art. 4. on account of the ſpheroidical figure of the earth being too inconſiderable to affect the concluſion.

§ 6. We are to obſerve, that the ſpace $\frac{3pmn\dot{z}^2}{2sT^2}$ deſcribed by that point of the equator, which is the interſection of the circle of the Sun's declination, is generated by the perpetual attraction of the Sun. This attraction may be reckoned conſtant during the very ſmall time of the earth's deſcribing $\dot{z}$ in its annual motion; and, therefore, the ſaid point of the equator, at the end of that time, will have acquired a velocity which would carry it through $\frac{3pmn\dot{z}^2}{sT^2}$ in the ſame time.

§ 7. Let T repreſent the time of the earth's revolution in its orbit, t the time of its rotation round its axis, and ſuppoſe $\dot{w}$ to be a ſmall arc ſimilar to $\dot{z}$ in a circle whoſe radius is unity. In figure 5. let AQ be the equator, and take Ab equal to $\frac{\dot{w}T}{t}$, and bt perpendicular to Ab equal to $3pmn\dot{w}^2$, and Ab, bt, will repreſent the directions and quantities of the two different motions of the point A, and conſequently At will be the direction of the

new equator, and as A*b* or A*t* is to *bt*, ſo is the radius unity to the ſine of the angle *t*A*b*, and if AQ or AG be taken equal to a quadrant, GQ the meaſure of the angle GAQ is equal to $\frac{3ppmnwt}{T}$.

§ 8. Suppoſe s the Sun's place in the ecliptic NS, N the equinoctial point, NA the Sun's right aſcenſion, and *ro* a perpendicular on AN: then *ro* is to GQ as the ſine of AN to the radius, and *r*N to *ro* as the radius to the ſine of the angle at N the inclination of the ecliptic to the equator, and, *ex æquo perturbatè*, *r*N to GQ as the ſine of AN to the ſine of N and *r*N the ſmall preceſſion of the equinoxes is equal to $3ppmn\,\frac{\dot{w}\times t}{T}\times\frac{\text{ſin. NA}}{\text{ſin. N}}$.

§ 9. In the ſpherical triangle ASN, the ſine of AN = cotang. N × tang. AS $=\frac{m\times\text{coſ. N}}{n\times\text{ſin. N}}$, and farther the ſine of SN $=\frac{m}{\text{ſin. N}}$ and *r*N is equal to $\frac{3ppt\times\overline{\text{ſin. }u}^{2}\times\dot{w}\times\text{coſ. N}}{T}$, whoſe fluent or $\frac{3pp}{4T}\times\overline{2w-\text{ſin. }2w}\times\text{coſ. N}$ gives the preceſſion of the equinoxes during the Sun's motion through the arc NS of the ecliptic: when NS is equal to a circle, then the whole fluent becomes equal to $\frac{3ppt\,A}{T}\times\text{coſ. N}$, and as 2T is to $3ppt\times\text{coſ. N}$, ſo is $60\times60\times360$ to $21''+6'''$ the annual preceſſion of the equinoxes in ſeconds produced by the Sun's attraction.

§ 10.

§ 10. We might now proceed in a ſimilar way to inveſtigate the effects of the Moon's diſturbing force, the rotation of the earth's axis, and the equation of the preceſſion; but ſince theſe propoſitions are purely mathematical, and the computations have already been gone through by other authors, it will be needleſs to repeat them here.

§ 11. NEWTON was the firſt who attempted to explain the preceſſion of the equinoxes from its cauſes. Since his time various other ſolutions have been given us by the moſt celebrated mathematicians; and it deſerves to be noticed, that, in a caſe where there can be little doubt that he was miſtaken, other authors have found it difficult to agree among themſelves in differing from him. M. D'ALEMBERT, in the year 1749, printed a treatiſe expreſsly on the ſubject, and has ſince ſaid [a], that himſelf is acknowledged to be the firſt who determined rightly the method of ſolving ſuch problems. EULER, DE LA GRANGE, FRISIUS, SILVABELLE, WALMESLEY, SIMPSON, EMERSON, have each conſidered the ſubject, and perhaps the importance of the enquiry would juſtify a minute examination into the cauſe of the agreement or diſagree-

(a) D'ailleurs, des géometres, vraiment capables d'apprècier mon travail, ont abondamment ſuppléé à tout autre temoignage, en declarant que j'ai ouvert le premier la route pour reſoudre ce genre de queſtions. See Opuſc. Math. Vol. V. ſur la Préceſſion des Equinoxes.

ment of their ſeveral methods; but I am deterred from entering into ſuch a diſcuſſion by the length of time which it would require; eſpecially as I think thoſe who have read the authors mentioned will eaſily conceive the ſubſtance of what I ſhould have to obſerve, and to thoſe who have not read them I ſhould hardly be able to ſay any thing intelligible.

§ 12. The above ſolution, if it had no other advantages, is, I apprehend, much more conciſe than any that has hitherto been given. Abſtracted from what is ſaid by way of illuſtration, articles 4th to 9th contain all the calculation requiſite, and as I have ſtudiouſly avoided the ambiguous uſe of the terms *force*, *vis*, *efficacia*, *momentum*, &c. as well as every doubtful repreſentation of times, ſpaces, and velocities, which are often ſubſtituted by authors in equations, I believe the whole proceſs will appear eaſy, and the evidence upon which the concluſion reſts be exactly aſcertained.

§ 13. The principles deſcribed in articles 2. and 4. depend upon the third law of motion, and the property of the lever, and are demonſtrated in the following manner. Every thing remaining the ſame as in art. 2. (fig. 6.) let AV and BR, perpendicular to the right line or axis AE, repreſent the forces and directions with which thoſe bodies are reſpectively urged, when at liberty to move

freely in thoſe directions; and let Av, Br, Cc, repreſent the accelerative forces of the reſpective bodies, as altered by their mutual actions upon each other: then, becauſe C×Cc is the moving force gained by C, and A×vV+B×rR the moving force loſt by A and B, regard being had to the lengths of the different levers AE, BE, we ſhall have A×vV×AE+B×rR×BE equal to C×Cc×CE, that is, A×AE×$\overline{\text{AV}-\text{A}v}$+B×BE×$\overline{\text{BR}-\text{B}r}$ equal to C×Cc×CE, and by tranſpoſition A×AE×AV+B×BE×BR equal to C×CE×Cc+A×AE×Ar+B×BE×Br. Let s, $\acute{s}$, repreſent, as in art. 2. the ſpaces which would be deſcribed by the bodies A and B at liberty in any very ſmall portion of time, and let x be the ſpace which A actually deſcribes in that time when connected with B and C by the lever AE. The quantities $\frac{x \times \text{BE}}{\text{AE}}$, $\frac{x \times \text{CE}}{\text{AE}}$, will then be the ſpaces deſcribed by B and C reſpectively; and, laſtly, becauſe the ſpaces deſcribed in given times are as the accelerating forces, the above equation gives x equal to $\frac{\text{A} \times \text{AE} \times s + \text{B} \times \text{BE} \times \acute{s} \times \text{AE}}{\text{A} \times \text{AE}^2 + \text{B} \times \text{BE}^2 + \text{C} \times \text{CE}^2}$.

The ſame method extends itſelf eaſily to more difficult caſes, and by its aſſiſtance ſeveral very important theorems are briefly demonſtrated.

§ 14. The reaſoning made uſe of in art. 6. will appear very evident to any one moderately verſed in the

elements of mechanics and the doctrine of moving forces; and therefore I muſt believe that it is by miſtake that one author of note entirely omits ſo neceſſary a ſtep which affects the concluſion by juſt one half. When a body moves with any velocity in the direction AM (fig. 7.) which would carry it through the ſpace AD in a ſmall particle of time, and any force which may be reckoned conſtant for that time urges the body through the ſpace DC perpendicular to AM, the body at the end of that time will arrive at the point C; but joining AC we are not to ſuppoſe, that, if that force ceaſed to act, the body would proceed in the direction ACL: for take C*m* equal and parallel to AD, and C*d* in CD produced equal to 2CD, and the direction of C at that point will be C*l*, the diagonal of the parallelogram C*dlm*.

Thus when a body revolves in any curve by a centripetal force (fig. 8.) we may, with Sir ISAAC NEWTON, ſuppoſe the curve to be compoſed of an indefinite number of right lines, and the body to move either in the chords or the tangents of the curve; but then we are to take care that we make not ſuppoſitions inconſiſtent with each other. Let the curve be a circle, and AD a tangent at the point A the direction of the body's motion when it arrives at that point, and let DC, parallel to the diameter AL be the effect of the centripetal force: then, if we

ſuppoſe the body to move along the chord AC, and ſay, that the angle CAD meaſures the deflection of the path in the time of the body's moving through the arc or chord AC, we ſhall miſtake by one half of the true quantity; for draw the tangent at c, and ſince A*d* is equal to *d*c from the property of the circle, the angle c*d*D of deviation is equal to twice the angle CA*d*. The practice of NEWTON in a ſimilar caſe, where he is inveſtigating the horary motion of the lunar nodes in a circular orbit, is entirely conſiſtent with this. See the Principia, lib. III. prop. 30.

§ 15. M. D'ALEMBERT has lately charged SIMPSON's account of the preceſſion of the equinoxes with ſome miſtakes of this nature in his ſecond lemma; but, in juſtice to SIMPSON, I muſt ſay, that, whatever other defects there may be in his paper, I am convinced, after the moſt diligent attention, that thoſe alluded to are without foundation.

§ 16. Sir ISAAC NEWTON firſt obſerved, that an homogeneous globe could not poſſibly retain many diſtinct motions, without compounding them all into one, and revolving with a ſimple and uniform motion about an invariable axis. When two forces impreſs upon a globe two diſtinct circular motions [(b)], he briefly concludes in

(b) See Principia, lib. I. prop. LXVI. coroll. 22.

his

his way, from the laws of motion, that it is the ſame thing as if thoſe two forces were at once impreſſed in the common interſection of the equators of thoſe motions, and upon this principle we ſuppoſed A*t* in art. 7. to be the direction of the new equator. In order to remove any doubts that might ariſe about the juſtneſs of this mode of compounding motion, FRISIUS has given a geometrical demonſtration of the principle: but the thing may be ſhewn much more eaſily in the following manner. Suppoſe (fig. 9.) RB, AB, to be two axes about which every point in the plane ABPR tends to move with velocities as the reſpective diſtances from the axes; let PQ perpendicular to AB be to PR perpendicular to RB as the angular velocity of P about RB to the angular velocity of the ſame point about AB, and let the velocities be in contrary directions: then, I ſay, every point in the plane will move with a velocity proportional to its diſtance from the axis PB. Firſt, it is evident, that any point C in the axis RB will move round PB with a velocity proportional to its diſtance C*m*: for the point C lying in the axis RB has no velocity round RB, and CM is proportional to CN. Draw P*c* parallel to AB, and any point D in that line will move with a velocity proportional to DV, which is perpendicular to PB, for the following reaſon.

The

The velocity of D is equal to the difference of its two velocities round the reſpective axes RB, AB, or the difference of D's velocity round RB, and P's velocity round AB, ſince all the points in PC move with the ſame velocity round AB. Draw DT parallel to CR, and this difference will be proportional to PT, becauſe the velocities of P round RB, AB, are ſuppoſed equal to each other, and PT is proportional to DV, and every point in the plane moves round PB with a velocity proportional to its diſtance; and the ſame thing may be ſhewn when any point is taken without the plane ABCPR.

§ 17. Becauſe any point C in the axis RB moves with the ſame velocity round PB as it does round AB, the angular velocities round the two axes AB, PB, will be to each other inverſely as their reſpective diſtances CN, C*m*; and becauſe CN : C*m* :: PB : PC and PR : PQ :: PC : CB, it follows, *that* PB *the diagonal of the parallelogram* PGBC *will repreſent the angular velocity of the revolving plane, when* BG, BC, *are taken to each other as the angular velocities of the ſame plane round thoſe reſpective axes.*

§ 18. From this it clearly follows, that the reaſon given by SIMPSON [c], in his miſcellaneous tracts, of the difference between his own ſolution and that of NEWTON in the Principia, cannot poſſibly be the true one. "It

(*c*) Pages 44. and 45.

"appears

" appears further," ſays he, " by peruſing his thirty-ninth " propoſition, that he there aſſumes it as a principle, that " if a ring encompaſſing the earth at its equator, but de- " tached therefrom, was to tend or begin to move about " its diameter with the ſame accelerative force or angular " celerity as that whereby the earth itſelf tends to move " about the ſame diameter through the action of the " Sun, that then the motion of the nodes of the ring and " of the equator would be exactly the ſame."

The principle is certainly implied in NEWTON's proof, and is capable of the moſt rigid demonſtration, art. 16, 17.

§ 19. It will be aſked then, where is the fault of NEWTON's reaſoning? How comes his concluſion to be too little by above one half? It is acknowledged on all hands that there is an error in his third lemma; but then the correction of that error makes only a very ſmall alteration in the reſult.

It is impoſſible for any one to form a complete judgement of his method without going through the whole of his calculations, which pre-ſuppoſes that the mean motion of the lunar nodes is computed. This motion may be conciſely determined and exactly enough for the purpoſe from prop. 30. of the Principia, and from thence is inferred the motion of the nodes of a ſatellite revolving

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in the plane of the equator at the ſurface of the earth with a velocity equal to that of the earth round its axis. We are then to ſuppoſe, that the *mean* motion of the nodes of a ſatellite, revolving with ſuch a velocity, is the ſame with the motion of the nodes of a ring of rigid matter ſurrounding the earth at its equator, and revolving with the ſame velocity. This laſt hypotheſis is admitted by SIMPSON, who thinks that his own ſecond lemma contains a full demonſtration of the point. For my own part, I believe with FRISIUS, that we are to look here for the material error in NEWTON's ſolution of the problem. It is evident, that the *true* motion of the nodes of the ſatellite, and the ring of matter, are not the ſame; and it is by no means obvious, that their mean motions are ſo. The mean motion of the nodes of a ring of hard matter cohering together is very eaſily computed by the method in art. 4th to the 9th, and turns out nearly double the mean motion of a Moon revolving at the ſurface of the earth with the ſame velocity.

It is a very intereſting enquiry to find out the real cauſe of the miſtake in the Principia, lib. III. prop. 39.; and therefore at a future opportunity I may, perhaps, conſider this particular part of the ſubject more attentively. I have long been ſatisfied with the account already given, and

ſhould probably have remained ſo, if M. D'ALEMBERT [d], in his Opuſc. vol. V. had not perſiſted to affirm, that the mean motion of the nodes of the ring of matter and of the ſatellite were the ſame.

This opinion of ſo celebrated a mathematician raiſes ſcruples in one's mind; and ſhews, that when we venture to differ from Sir ISAAC NEWTON in theſe matters, it is with the utmoſt difficulty that we can arrive at certainty.

(d) Il n'y a de parité que dans le mouvement *moyen* de ces deux anneaux, ou de l'anneau ſolide et de la lune; les mouvemens *inſtantanés* ſont très differens de part et d'autre; ainſi la comparaiſon du mouvement de l'anneau avec celui de la lune, ſerviroit tout au plus à trouver le mouvement *moyen* de l'anneau, ou de la préceſſion des équinoxes, mais nullement à déterminer la nutation de l'axe et l'équation de la préceſſion.

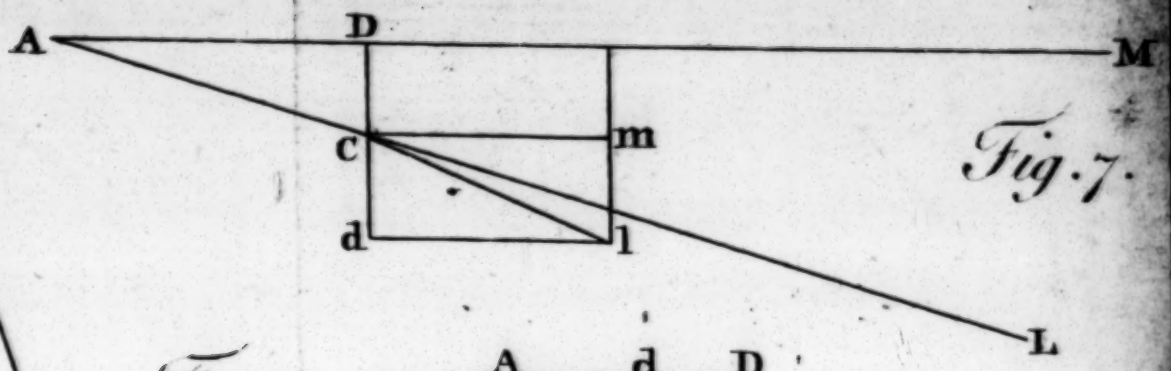


Fig. 7.

Fig. 8.

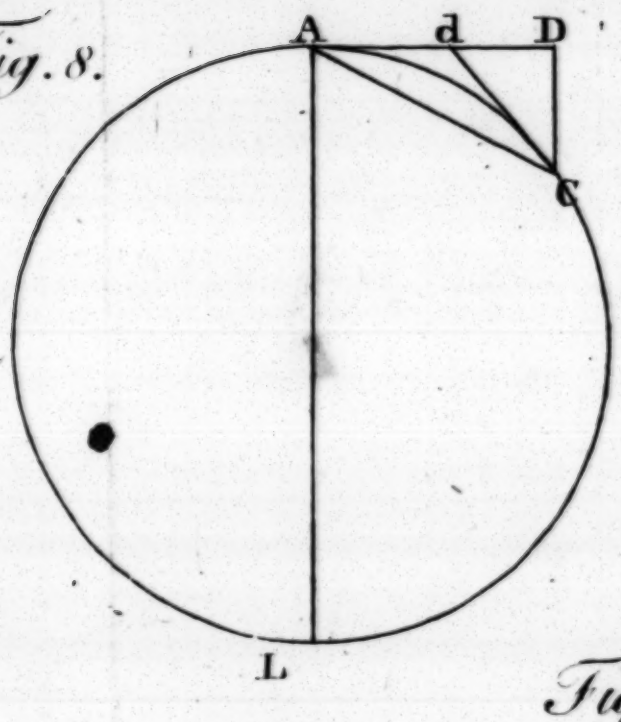


Fig. 9.

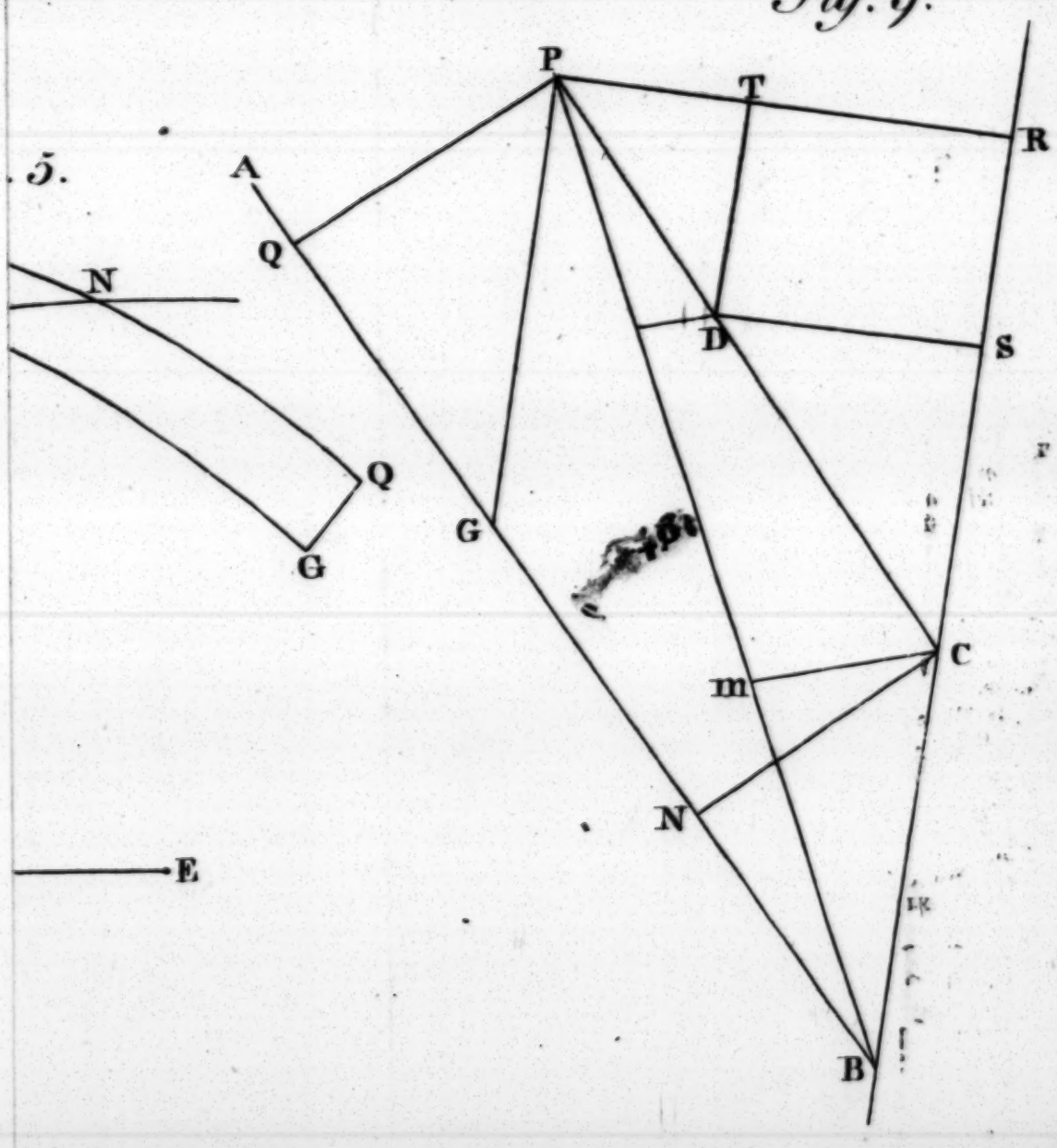


Fig. 1.

Fig. 2.

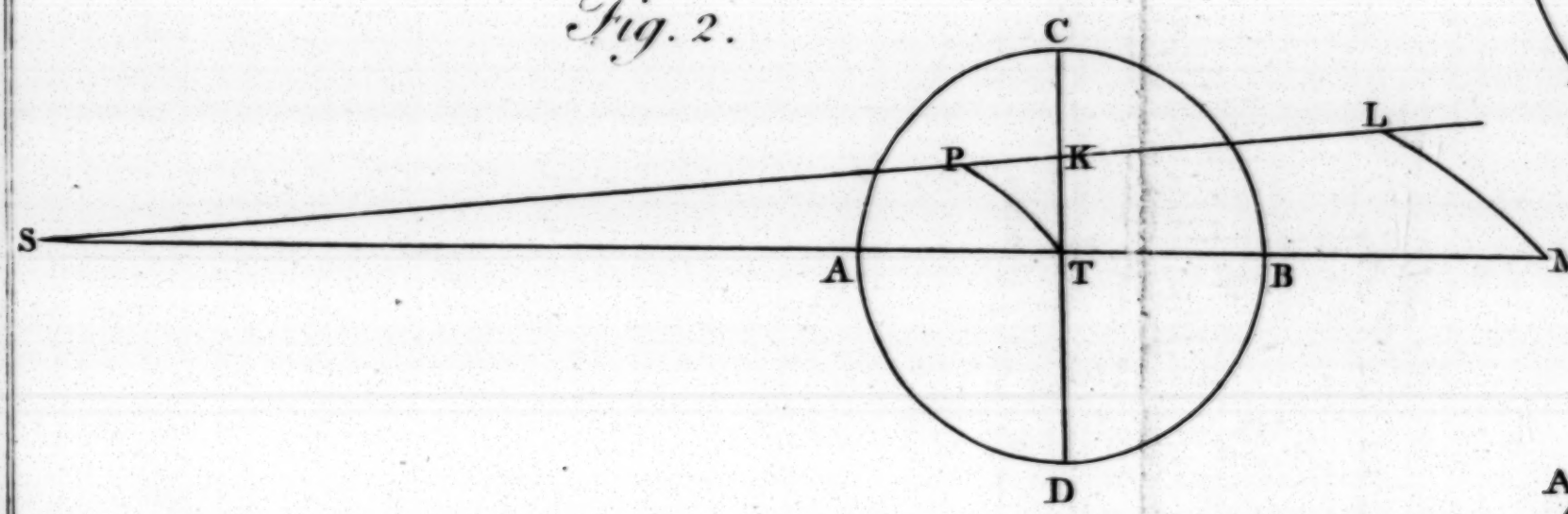


Fig. 3.

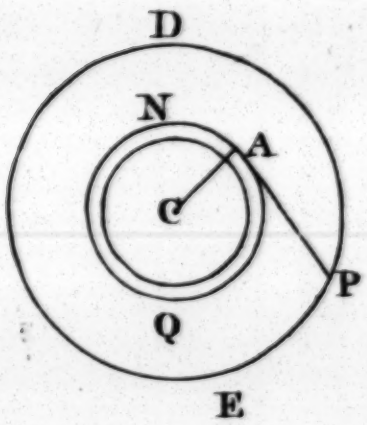
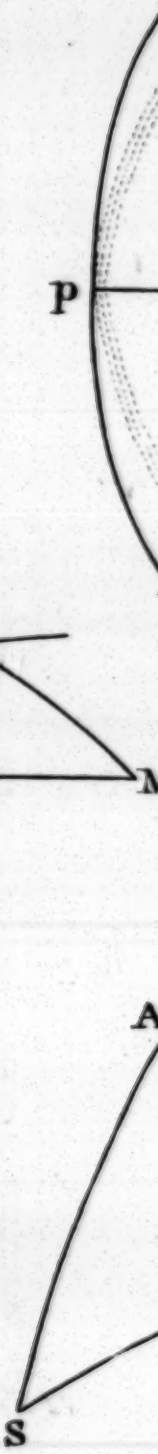
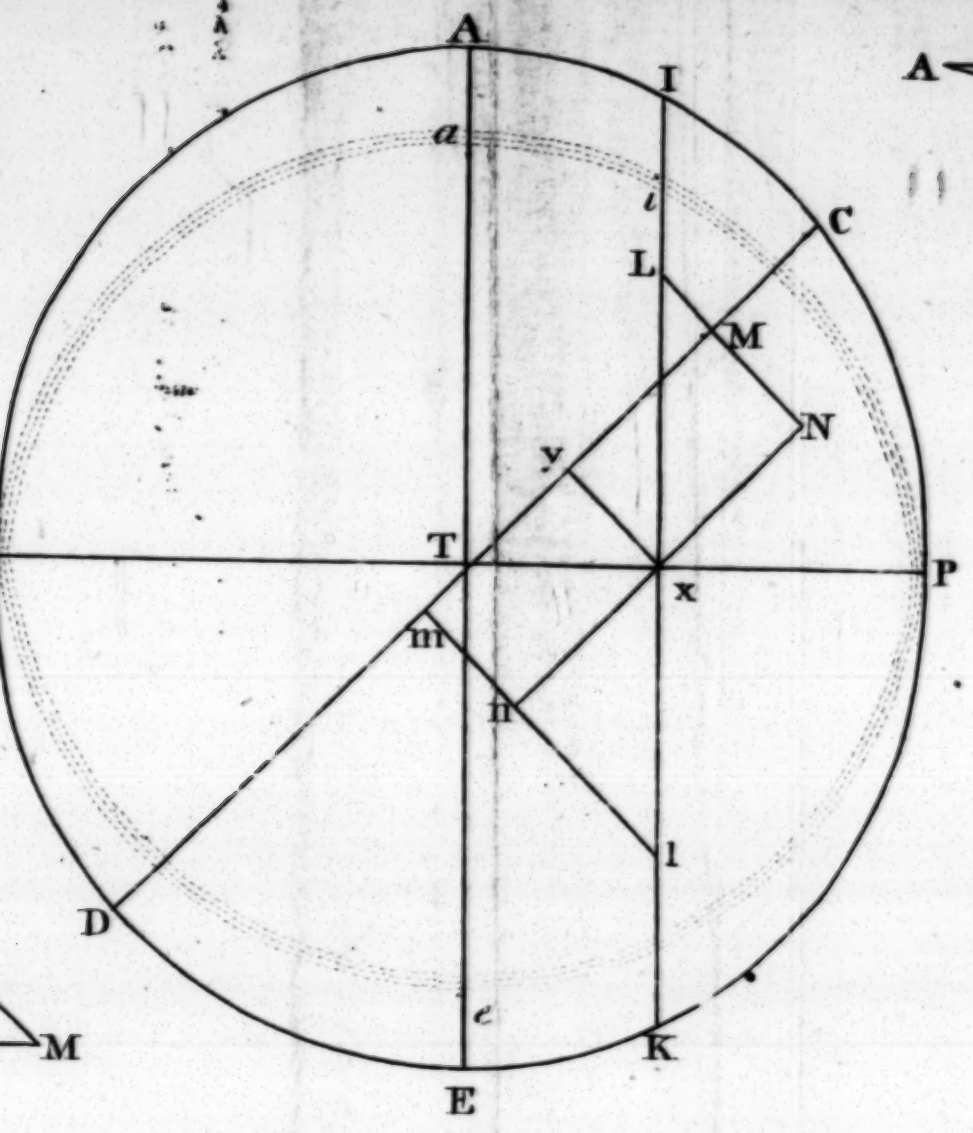


Fig. 4.





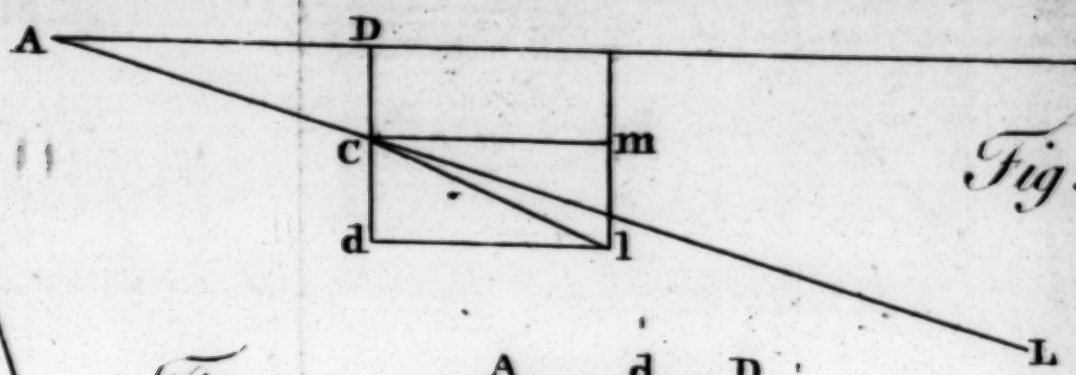


Fig. 7.

Fig. 8.

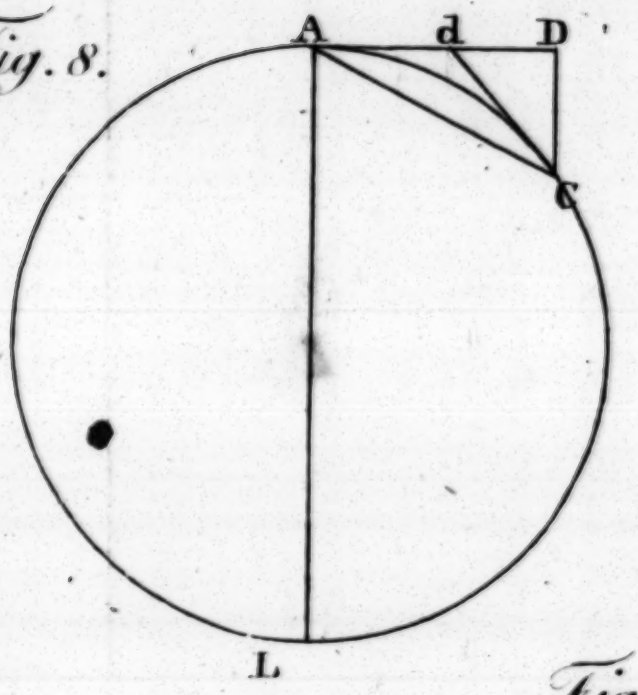


Fig. 9.

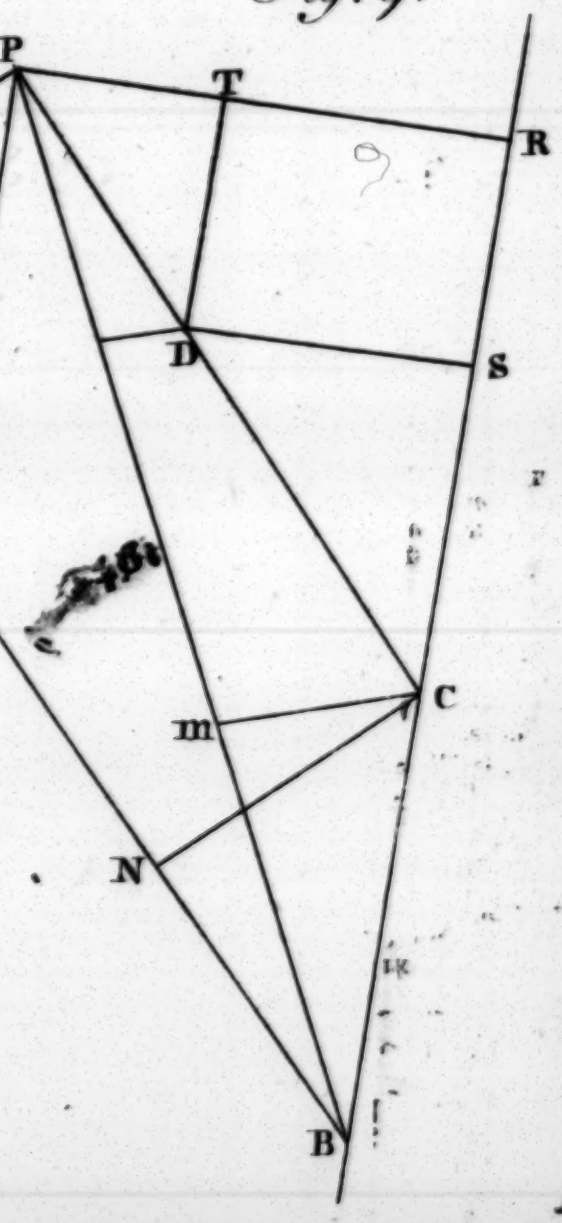


Fig. 5.

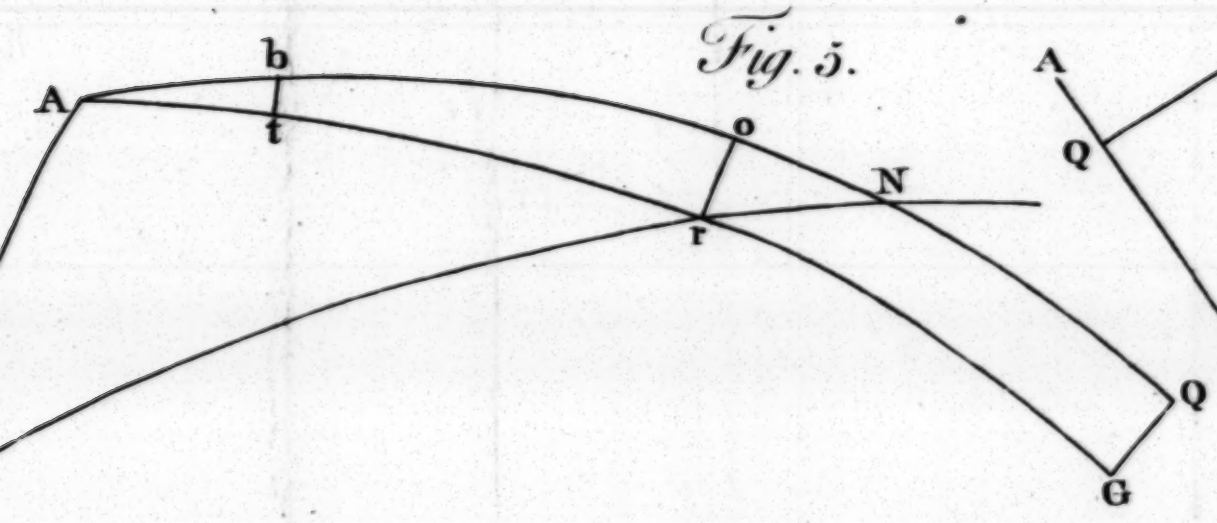
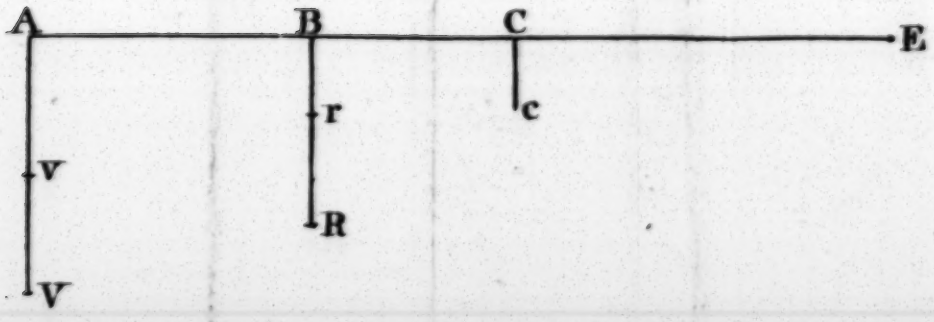


Fig. 6.



XXXIII. *An Examination of various Ores in the Muſeum of Dr.* William Hunter. *By* George Fordyce, *M. D. F. R. S. and Mr.* Staneſby Alchorne.

Read May 20, 1779.

MINERS and aſſay-maſters being generally employed in finding out metals for profit, without inquiring into the ſtate or combination they are in, have made uſe of proceſſes adapted, in many caſes, to inveſtigate the quantity of metal, gueſſing often at its ſtate or combination; and ſpecimens being procured from them for cabinets, an erroneous account has not only been given of their contents, but theſe errrors have alſo crept into books of natural hiſtory: for this reaſon Dr. FORDYCE determined to aſſay many of the doubtful ſpecimens, by proceſſes better adapted to find the ſtate or combination of the metal than thoſe commonly in uſe; and procured the aſſiſtance of his friend Mr. ALCHORNE. The reſult of ſome of their inquiries they now lay before the Society.

The firſt ore we mean to conſider is called gold pyrites. Pyrites is an ore of iron, containing ſometimes copper, ſometimes arſenic, ſometimes other metals; generally, if not always, combined with ſulphur.

This ore, from its yellow colour, has at firſt ſight been often taken for an ore of gold. It is the moſt common of all ores, and on examination is very ſeldom found to contain gold. Specimens, however, have been found from which gold has been procured; and, particularly, there have been found in Fatchobuigna near Zalatna, in Tranſylvania, maſſes which contain a large proportion of this metal. Sometimes the gold is in its metallic form, and viſible to the naked eye; ſometimes it is not: and in theſe caſes the ore has been thought to contain the gold united firſt with iron, and that compound united with ſulphur.

Dr. FORDYCE obſerves on this propoſition, that it has not as yet been proved, that a compound metal can be combined with ſulphur. If two metals are ſoluble in ſulphur, and each be ſeparately combined with it, the two compounds may be diffuſed through one another, as is the caſe with the compounds of ſulphur and iron, and ſulphur and copper; which may be diffuſed through one another: but if we have a compound of two metals, of which one is ſoluble in ſulphur, and the other is not, if

we

we apply ſulphur to this compound, it will diſſolve the one, and leave the other; as, if gold and ſilver be combined, if we reduce the maſs to fine particles, and mix them with ſulphur, and throw them into a crucible, heated white hot, and afterwards melt the whole maſs, the ſilver will combine with the ſulphur, and the gold will fall to the bottom of the veſſel. This being the caſe, we doubted whether this ore contains the gold in its metallic form, only mechanically mixed with the pyrites; or combined with the ſulphur by means of the iron: and therefore ſubjected a ſpecimen to examination, in which we could not diſcover, even by the help of a microſcope, any particle of native gold.

EXP. 1. We powdered one hundred grains of this ore, and boiled it in nitrous acid diluted with water; a ſolution took place with efferveſcence. Having digeſted them together, till the whole ſoluble part was taken up, and poured off the ſolution, and made a precipitation by fixed vegetable alkali, the precipitate appeared to be iron. Having waſhed the remaining part with water, and expoſed it in a glaſs matraſs in ſand, to nearly a red heat, a very ſmall portion of ſulphur ſublimed; the remainder was quartzoſe ſand, with particles of gold, which were ſimilar in figure, though ſmall, to the particles of gold found native in veins mixed with various matrixes, and

not at all like particles which had been combined with a *menſtruum*, which ought either to have appeared in a powder, whoſe particles were hardly viſible from their ſmallneſs; or in cryſtals ſimilar to one another.

EXP. II. If any metal be combined with ſulphur, mercury will not precipitate the ſulphur from it; we therefore took 140 grains of the ſame ore, and triturated it for ſome hours with about five times its weight of mercury: the powdered ore was waſhed off from the mercury, the remainder put into glaſs veſſels, and evaporated by heat: a maſs of gold was left, but part of it being accidentally loſt, its weight could not be aſcertained; it did not amount to above two or three grains at moſt.

The powder, waſhed off and dried, weighed 134 grains: theſe, mixed with as much litharge, and four times their weight of fixed vegetable alkali, and one fifth of wheat flower, and the whole melted produced a regulus of lead, weighing 80 grains, which, on coppelling with a few grains of ſilver, and parting in *aqua fortis*, left one ſixteenth of a troy grain of gold.

It is to be remarked, that in great works, where gold is ſeparated from pieces of crucibles, ſand, or other matter, by amalgamation, notwithſtanding the proceſs be

frequently repeated, and with great care, ſome ſmall portion of the gold ſtill remains; ſo that the ſmall quantity left in this caſe might eaſily have either eſcaped the mercury, or, have been left unobſerved in the powder combined with ſmall particles of it.

We may therefore conclude, that in this ore the gold was in its native form, and not mineralized.

EXP. III. We examined gold pyrites mixed with quartz, with a deep magnifier, and found evidently native gold interſperſed. One hundred grains of this ore, was powdered; and boiled in nitrous acid diluted with water: a ſolution of iron took place, as in the firſt experiment. The *reſiduum* being expoſed to a red heat, there was no appearance of ſulphur; the gold was found in ſuch large particles, and ſo ſimilar to native gold, that we did not think it worth while to apply mercury.

The gold being diſſolved out by *aqua regia*, a quantity of quartz and arſenical ſalt was left.

EXP. IV. When gold and ſilver are found in their metallic form, it is not uncommon to find them mixed; but it ſeldom occurs, that they are mixed in ſo large proportion as in an ore obtained from Norway, and which was given to Dr. HUNTER, by Mr. FABRICIUS.

The

The ore has the appearance of native ſilver, in ſcaly particles, intermixed with a hard quartz, tinged brown in ſome parts by iron. Twenty-five grains of the maſs, where the metal was in the largeſt proportion, melted with litharge, alkali, and phlogiſtic matter, and afterwards coppelled, yielded a globule apparently ſilver weighing two grains which, being boiled in nitrous acid diluted with water, left full nine ſixteenths of a grain of fine gold. Hence one hundred pounds of metal obtained from this ore, conſiſts of ſeventy-two pounds of ſilver, and twenty-eight pounds of gold.

There is an ore of ſilver which is commonly called vitreous (*minera argenti vitrea*, or *argentum vitreum*).

This ore has always been ſuppoſed to conſiſt of ſulphur and ſilver; becauſe, if we melt ſulphur and ſilver together, they form a maſs which reſembles it, eſpecially in colour and malleability; but, as we could find no experiment in any author which authorized this conjecture, we determined to endeavour to aſcertain it by analyſis, Dr. HUNTER not refuſing to ſubject the ore to an aſſay, although ſcarce and expenſive.

EXP. V. Fifty grains of this ore broken in pieces, for it is too malleable to be powdered, were boiled in nitrous acid diluted with water; the acid diſſolved the ſilver with much difficulty, and left a *reſiduum*. The ſolution being poured

poured off, and the *reſiduum* waſhed and expoſed in a glaſs matraſs to a red heat, there was no ſublimate of ſulphur, or any thing elſe obtained. The *reſiduum*, before it was expoſed to heat, did not appear to contain any ſulphur; being of a blackiſh colour after expoſure to heat, it had a number of yellow particles mixed, which eaſily diſſolved in *aqua regia*. Tin being applied to the ſolution made no precipitation, which it would have done if thoſe particles had been gold; hence we ſuppoſed they were iron.

The ſilver was precipitated from the acid by copper: waſhed and dried it weighed four grains, which, on coppelling with lead, loſt one twenty-fourth.

There were particles of powdered quartz apparently mixed with the yellow particles left after the ſolution.

EXP. VI. Being much diſappointed in not finding any ſulphur in the former experiment, we took three hundred grains of the ſame ore. We freed it as well as poſſible from heterogeneous matter: we mixed it with four times its weight of mild fixed vegetable alkali, put them into an earthen body, to which was affixed a glaſs head, and expoſed them to nearly a white heat in a naked fire for a full hour and an half; but no ſublimation took place, there diſtilling only a few drops of water. We then put the body into a melting furnace, and rendered

the whole fluid; as ſoon as it was melted, it was removed from the fire.

The maſs, on concreting, was found divided into two; a black maſs a-top, and a metallic maſs at bottom. The metal being aſſayed by ſolution in nitrous acid and precipitation with volatile alkali, ſhewed no ſign either of gold or copper; loſt nothing by coppellation with lead; it weighed 213 grains, which were pure ſilver.

The black maſs a-top, or *ſcoriæ*, was boiled repeatedly in water, but did not all diſſolve. The inſoluble part was unfortunately thrown away; but to the ſolution we added muriatic acid: on the addition of the acid, there was a ſtrong ſmell of *hepar ſulphuris*, and a copious precipitate, which, on being examined by a microſcope, appeared to conſiſt of pellucid cryſtals, without the ſmalleſt appearance of ſulphur. This precipitation, being expoſed to heat, did not ſmell in the leaſt like ſulphur; it was not in the leaſt inflammable. Excepting then the ſmell of *hepar ſulphuris*, there does not appear any mark of ſulphur in this ore, and a very ſmall particle of inflammable matter dropping in by accident would give this ſmell.

The foregoing experiments occaſioning ſome doubt of ſulphur's being contained in vitrous ſilver ore, we endeavoured to inveſtigate it by other means; and after ſeveral

experiments,

experiments, we made the following one, which ſeems concluſive.

Half an ounce of ſilver was precipitated from nitrous acid by copper, in ſmall flaky cryſtals as uſual; being waſhed and dried, it was mixed with the ſame weight of ſulphur, and put into a crucible, over which another was placed ſo as to cover it, and the two crucibles were luted together, leaving a ſmall aperture for the eſcape of vapour, but not ſo as to admit air for the inflammation of the ſulphur. Theſe being put into the fire, as ſoon as they were heated red-hot, the ſulphur in part eſcaped through the ſmall hole, and as it paſſed through burnt with a blue flame. The fire was increaſed to a ſufficient degree to melt the maſs within; in the mean time the blue flame diſappeared, which ſhewed that no more ſulphur eſcaped.

After we had applied heat enough, as was ſuppoſed, to melt the maſs, the crucibles were removed from the fire, and being ſeparated when cold, a regular button-like maſs was found in the crucible, of a dark lead colour both without and within, brittle, and ſtreaked on the inſide, bearing to be cut with a knife, exactly like the vitreous ore, which it in every way reſembled. The whole maſs gained twenty grains or one twelfth in weight, which may be conſidered as the true weight of the ſul-

phur in this compound, as ſilver combines with ſulphur in its metallic form, without loſing its inflammable air. Fifty Troy grains of this compound was powdered with difficulty, from its toughneſs, and boiled in nitrous acid diluted with water; it diſſolved with difficulty, leaving a very ſmall quantity of a light blackiſh powder, certainly not nearly a grain in weight.

It appears, therefore, that the ſmall portion of ſulphur contained in the vitreous ore may have been decompoſed by the nitrous acid, and the alkali, or the heat, in the former proceſſes; and we may conclude, that vitreous ſilver ore is a compound of ſilver and ſulphur, and when pure, that it contains between ninety-two and ninety-three grains of ſilver in one hundred.

XXXIV. *On ſome new Methods of ſuſpending Magnetical Needles.* *By* John Ingen Houſz, *Body Phyſician to their Imperial Majeſties, and F. R. S.*

Read June 17, 1779.

THE very great utility which navigation derives from the uſe of compaſſes, has been the principal reaſon, that ſo much labour has been beſtowed by many ingenious men in ſearching the beſt method of ſuſpending magnetical needles, and that ſome Academies of Sciences have propoſed conſiderable premiums to be given to him who ſhould ſucceed the beſt in this important object.

It has been, and is ſtill, a general complaint, that ſuch needles as were by their ſize and figure ſuſceptible of the greateſt magnetical power, and which were executed with the greateſt exactneſs, and perfectly balanced, were, for this very reaſon of their ſuperior accuracy, ſo eaſily put in motion, that the ſlighteſt ſhaking of the floor, occaſioned by the obſervers walking in the room, communicates to them ſuch a great quivering motion as to drag them out of their direction, and to render a conſiderable

time

time requiſite before they are again fixed in the true magnetical meridian.

Since the late Dr. KNIGHT has improved ſo greatly the method of communicating a conſiderable magnetical force to ſteel bars, needles have been impregnated with a much greater and more permanent polarity than it was poſſible to give them before, and the conſtruction itſelf of compaſſes has been conſiderably improved. But the ſubject ſeems not yet to be exhauſted.

The degree of magnetical power or polarity to be communicated to ſteel well hardened ſeems to depend in ſome reſpect on the proportion which the ſurface of the piece of ſteel to be impregnated bears to its bulk; ſo that a thin lamella of ſteel, weighing for inſtance ten grains, will acquire more magnetical force than a little lump of ſteel of the ſame weight. Thus a ſmall magnet, compoſed of a great many thin lamina of ſteel preſſed together, may be made ſo ſtrong as to ſupport 150 times its own weight, and even more, which has never yet been done by a compound heavy magnet.

If the too great quivering and horizontal motion, or the too great reſtleſſneſs of a ſtrong magnetical needle, is in ſome way or other counteracted by the methods hitherto adopted, the needle is in ſome danger of ſtopping near the true magnetical meridian, without always

pointing

pointing directly in it; becauſe near this very place the power of direction of the needle may be ſo weak as not to overcome the reſiſtance, which was oppoſed to its free horizontal circular motion.

I thought, in the courſe of laſt year, that a great part of the above mentioned difficulty might be taken away by different methods, ſome of which are the following.

EXP. I. I placed an ordinary magnetical needle, ſupported upon its point, in a china baſon, and poured water into it, ſo as to cover the whole needle. The needle loſt a great deal of its reſtleſſneſs, was influenced by the approach of a magnet at a conſiderable diſtance, and ſeemed to point as well as before to the magnetical meridian, though with a ſlower motion.

EXP. II. I took a ſtrong flat magnetical needle, having in its center a round hole, but no cap. I fixed to this needle as much cork as was neceſſary to make it juſt ſwim upon the water in a baſon. I fixed a ſmooth braſs pin in a vertical poſition, ſo that it paſſed through the hole in the center of the needle, to prevent its ſwimming to the ſides of the baſon. This did tolerably well: the needle moved to the magnetical meridian with a ſlow motion.

EXP. III. I then took the needle uſed in the firſt experiment, and fixed upon it as much cork as was requiſite juſt

juſt to make it ſink. I placed a point under it, ſo that the needle with the cork was kept a little below the ſurface of the water, bearing upon the point with a very inconſiderable weight. This anſwered nearly as in EXP. II.

EXP. IV. I fixed to the center of a ſmall, but ſtrong, magnetical needle a ſilver wire; to the other extremity of this wire I fixed a ſmall ſteel-working needle, very much hardened. I ſtuck the point of this ſewing needle to the lower extremity of a ſteel magnet, placed in a vertical poſition and highly poliſhed. This whole apparatus was placed in a glaſs cylindrical veſſel, filled with water ſo far as to cover the point of the working needle. The horizontal magnetical needle, thus ſuſpended, was remarkably quick in directing itſelf into the magnetical meridian, and was very eaſily diſplaced out of its direction by the approach of a magnet at a conſiderable diſtance.

The greateſt difficulty I found in this contrivance was, that a little jerk ſhook the point of the ſewing needle from the vertical magnet; and that a heavy magnet could not be ſuſpended from the point of a ſewing needle, but required a tolerably good magnet rounded at its extremity, where it was ſuſpended from the vertical magnet, or a thick piece of iron rounded in the ſame manner, by which means, however, the horizontal

magnet

magnet did not move ſo freely. The firſt difficulty could be eaſily diminiſhed by letting the ~~work~~*ſew*ing needle paſs through a ſmall and ſmooth hole in a glaſs or metallic plate, to controul its vibrating motion, and by adapting to it a ſhoulder, by which it ſhould ſtop upon the glaſs or metallic plate, and from which it ſhould be taken up again, by ſliding the vertical magnet a little down ſo as to touch the point of the ~~work~~*ſew*ing needle, and to lift it up again into its former place. The ſecond difficulty could, I think, alſo be in a great meaſure obviated by employing, inſtead of a common magnetical needle, a thin ſteel tube (of which I will ſpeak by and by) which is only a little ſpecifically heavier than an equal bulk of the liquid. By this means a ſmall vertical magnet could be made to ſupport a large one, even of a conſiderable abſolute weight, of which weight, however, the vertical magnet ſhould ſupport almoſt nothing, it being counter-balanced by the fluid in which it is immerſed.

In the experiment, as I had made it, I could not perceive, that the horizontal magnetical needle was influenced by the vertical magnet, which would probably be the caſe if the vertical one was much out of the vertical line, and too near the horizontal needle.

EXP. V. I adapted to a flat ſtrong magnetical needle two caps, turned one againſt another, ſo that the needle

could be ſupported on either of the caps, and turned with either ſurface upwards. I fixed on one of the flat ſurfaces, on each ſide of the center, a thin glaſs tube, hermetically ſealed, of ſuch a ſize, as made the whole together dip in the water ſo deep that only a ſmall part of the glaſs tubes remained above the ſurface of the water. I then depreſſed the needle entirely under the ſurface of the water by thruſting a metallic point in the cap which was uppermoſt, and fixing this point to the cover of the baſon: thus the needle, kept under water, bore againſt the point only with that ſmall degree of preſſure which the very inconſiderable difference of ſpecific gravity, which it had above an equal bulk of the water, could give it, which did not amount to above a few grains of weight. Thus this needle, loſing nothing of its polarity, loſt very near the whole of the reſiſtance of its weight, and at the ſame time that quivering motion and reſtleſſneſs, which it had in the air, moving ſmoothly in a medium, which could only obſtruct its too great vibrations (almoſt in the ſame way as aſtronomers ſtop the vibrations of their plumb line by hanging the weight in water or oil) without obſtructing (but retarding only) its tendency to the magnetic meridian, liquids preſſing equally on all ſides.

To

To prevent this needle from being ſhook off the point, and wandering about in the baſon, I placed alſo a metallic point underneath, upon which the under cap muſt bear if the needle ſhould by any cauſe deſcend from the upper point, or if I ſhould chuſe to make the whole ſink by increaſing its weight.

Having now found, by experience, that a ſtrong magnetical needle did point to the magnetical meridian near as well under water as in the open air, and that by the reſiſtance of the medium much of its too great verſatility was taken away; I wanted to try, what degree of magnetiſm could be given to a thin ſteel cylindrical tube, as a needle made in this ſhape could be as light as required without being incumbered with cork or glaſs tubes; but finding no ſuch tubes ready made, I tried one made of tin and found it ſuſceptible of a much greater magnetic virtue than I expected, conſidering that the iron plates of which it is made are neither ſufficiently hardened for this purpoſe, nor approach enough to the nature of ſteel; beſides that, being covered with a pewter coating, they cannot be expoſed to the bare friction of a ſteel magnet.

This experiment, however, afforded me a certain proof, that a magnetic needle, made of a thin ſteel tube,

would be ſuſceptible of a ſtrong polarity, ſo as to ſerve for the purpoſe here intended.

But as the two agate caps, if fixed in the middle of the ſubſtance of the tube, would interrupt ſome part of the continuation of the ſteel, and thus leſſen the magnetic power, the points of ſuſpenſion would be better ſoldered or fixed upon the ſurface of the tube itſelf, and the agate caps fixed upon the ſupport underneath and above the tube; ſo that ſuch a needle ſhould be the reverſe (in reſpect to the ſuſpenſion) of the common needles. Another advantage would be derived from fixing the points of ſuſpenſion upon the needle itſelf, *viz.* that by the motion of the fluid in which it ſwims, it would be leſs apt to acquire a too ſtrong waving or undulating motion.

If it ſhould be found difficult to make very thin ſteel tubes properly hardened, a piece of ſteel could be ſcooped out ſo as to conſtitute the half of a tube; the other half could be made of another ſimilar piece, or perhaps better of thin braſs, or any other metal, and they might be ſoldered togther.

It would, perhaps, anſwer the ſame purpoſe if a ſteel magnet was ſhut up in a thin tube of glaſs or ſome metal.

I ſhould

I ſhould think, that there could be no great difficulty to adapt to theſe kind of needles a thin glaſs, metallic, or enamelled plate, ſerving inſtead of the card in the common nautical compaſſes, and to make uſe of them in azimuthal compaſſes.

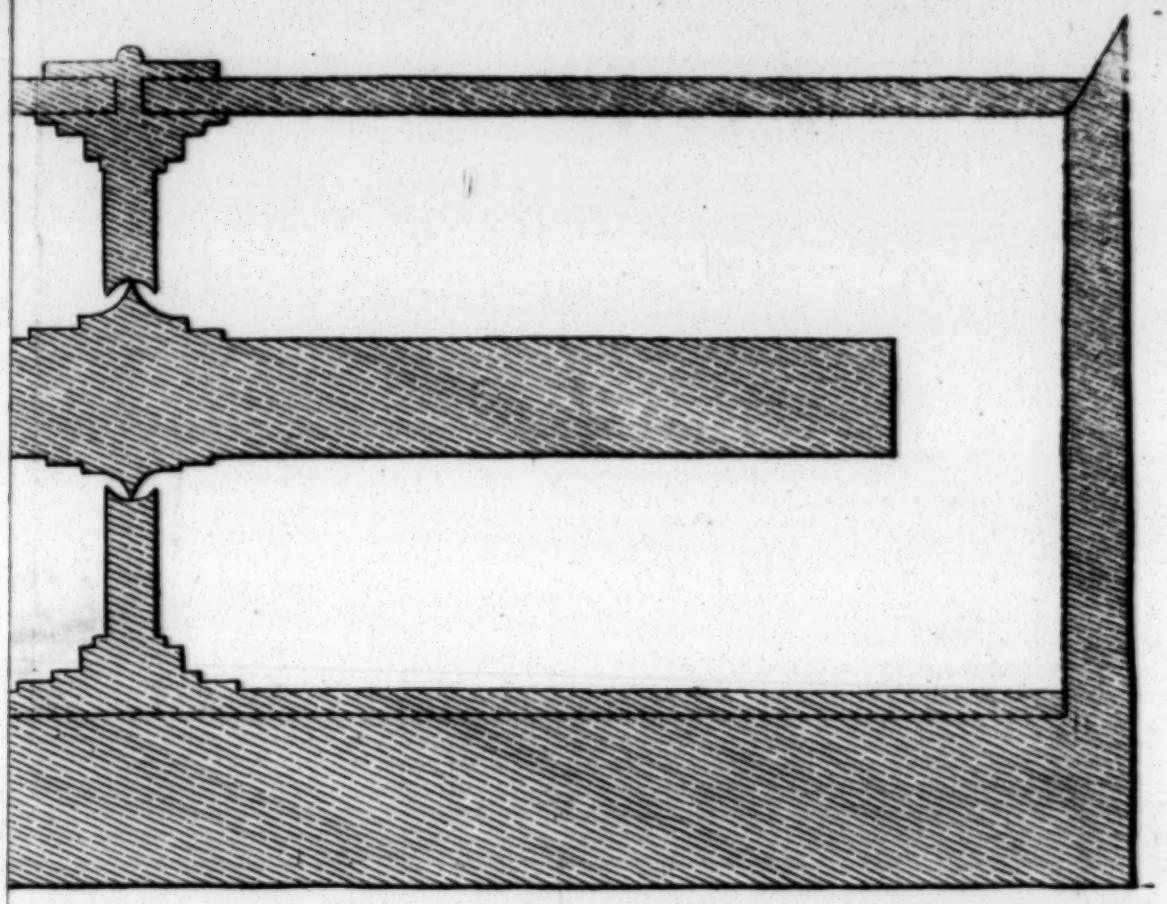
A card, if it is not required to be ſtuck upon the needle, might be fixed to the bottom of the glaſs baſon deſtined for this apparatus.

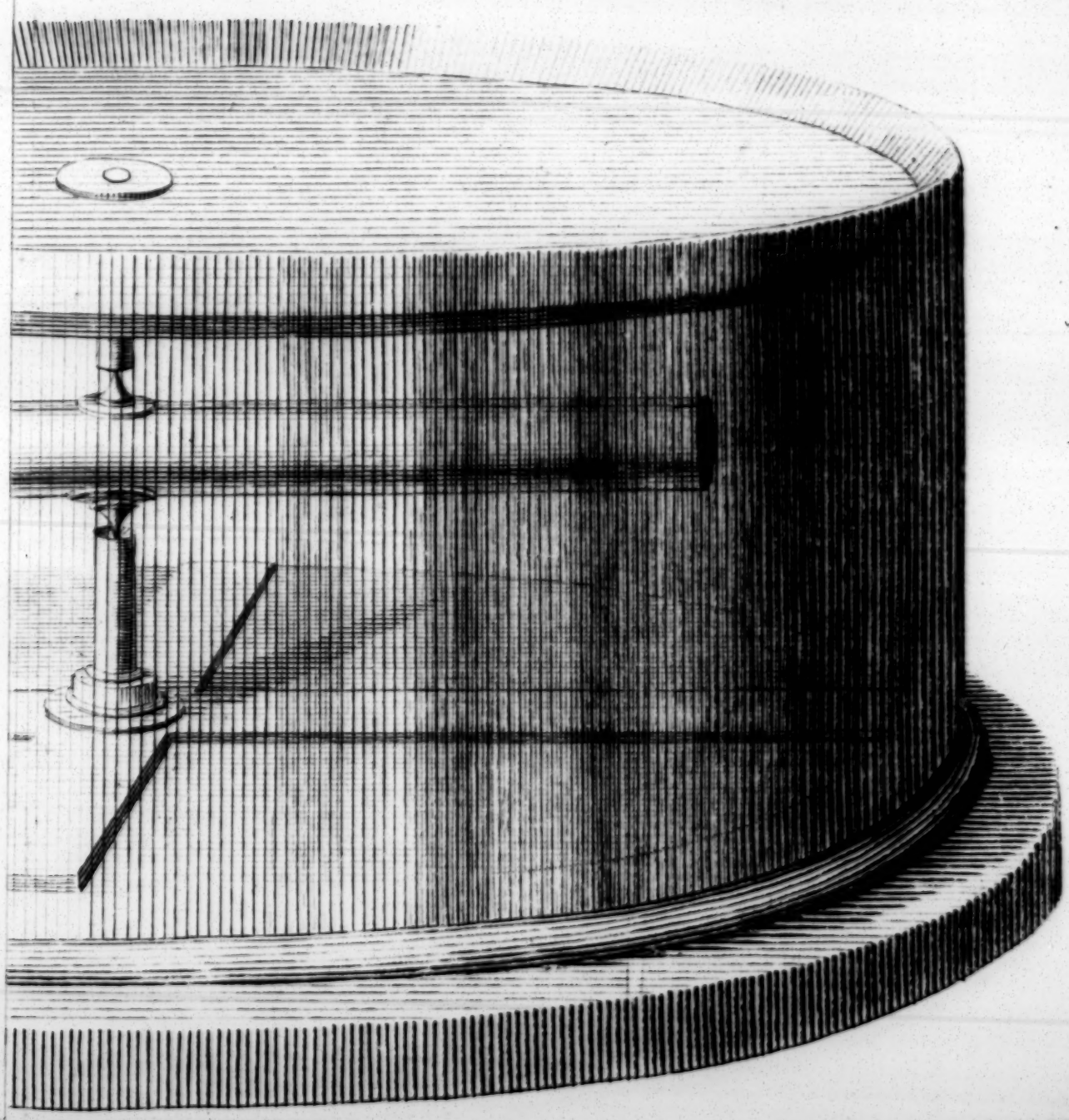
Common water would be, perhaps, the beſt medium for theſe different contrivances, if ſteel was not ſo eaſily ruſted by it, if the needle was covered with ſome varniſh or metal unalterable in water, and if in cold weather it was not ſo apt to freeze; and therefore, I think, ſome of the thinneſt expreſſed oils would anſwer the purpoſe better. Linſeed oil, when it has ſubſided, is very clear and tranſparent, and is not ſubject to freeze, at leaſt in the common cold of our climates; beſides, it increaſes the magnetical power of ſteel, as Dr. KNIGHT found. If it thickens by time, it may be changed.

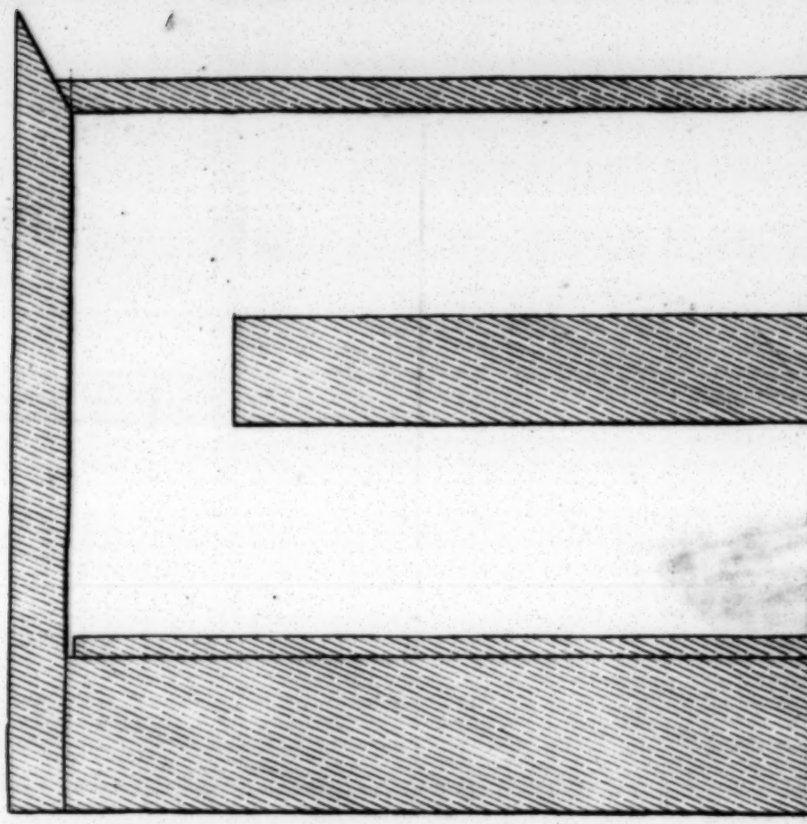
The glaſs baſon containing ſuch a compaſs ſhould be kept full of the liquid to the cover, to obſtruct undulating motions.

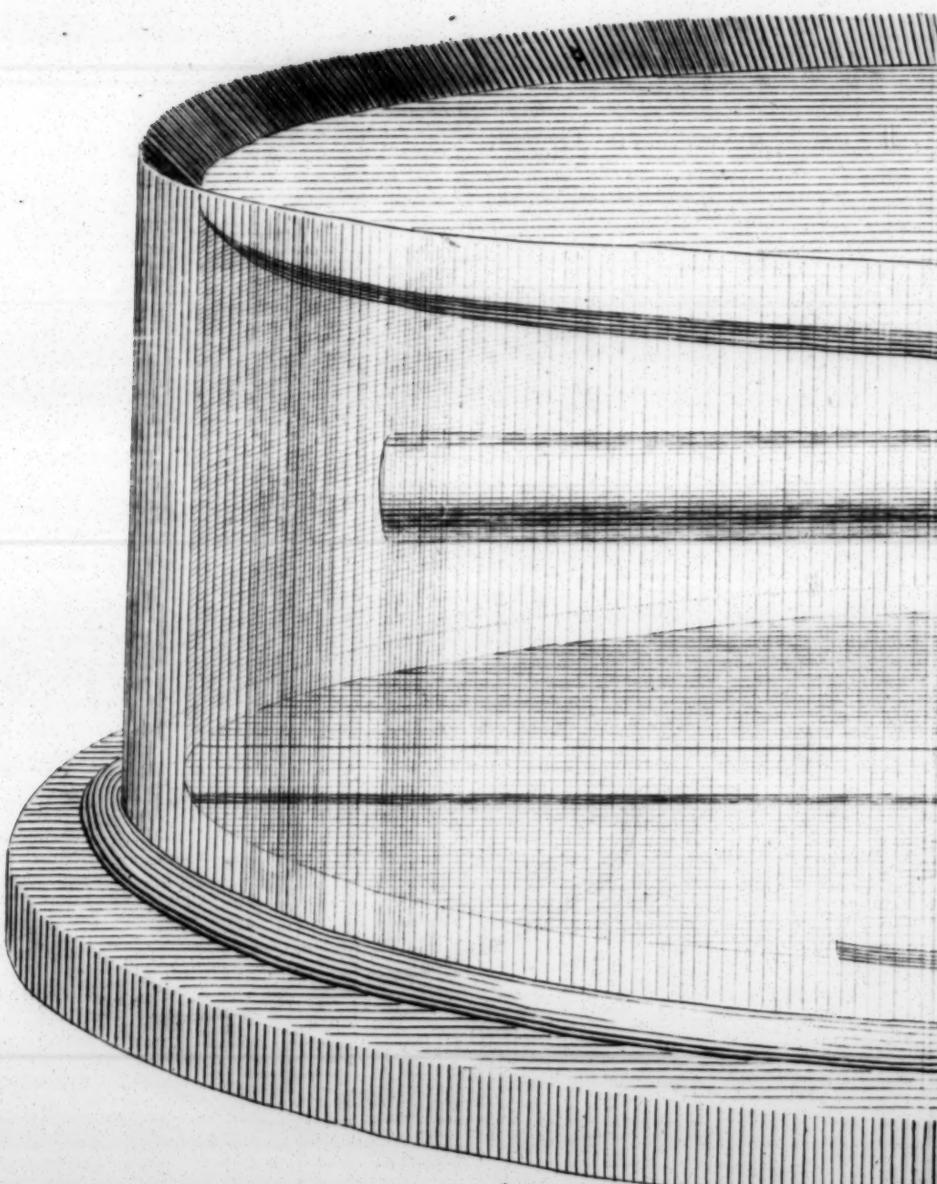
The plate joined to this paper is not made to ſerve as a model of a compleat apparatus, as the drawing was made from

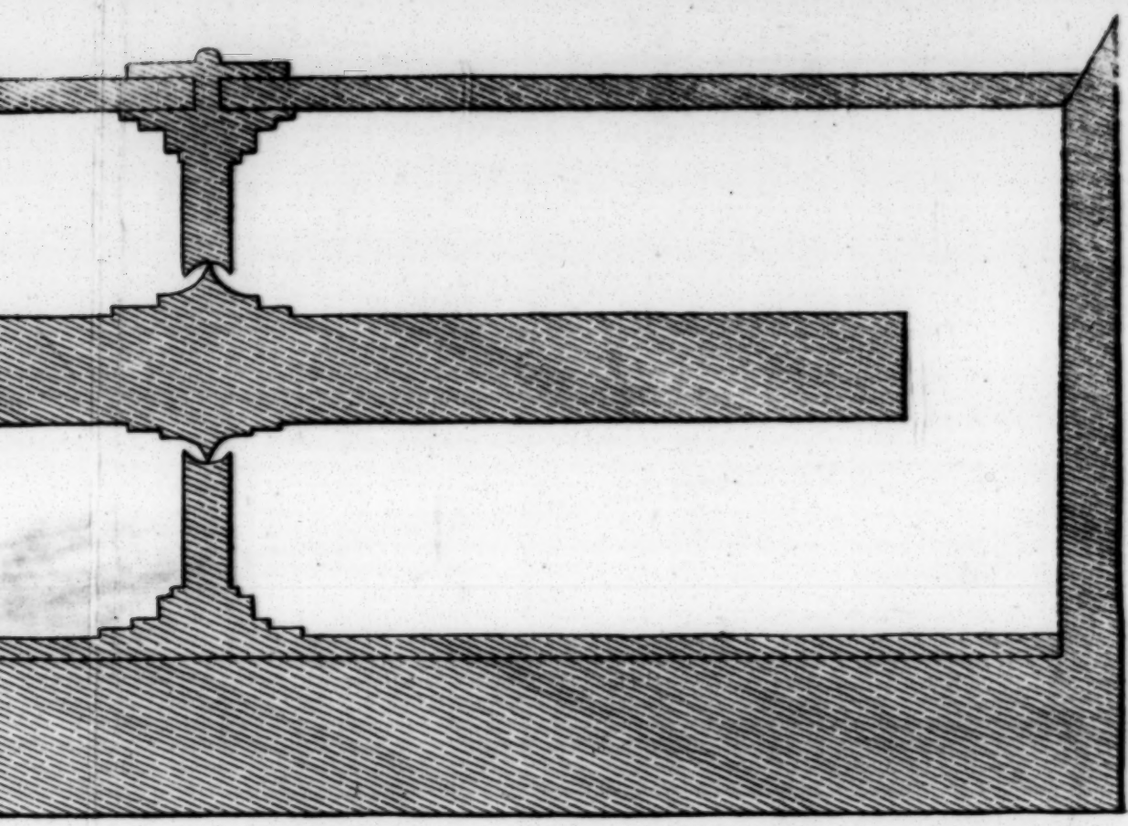
from fancy. It is deſtined only to convey an idea of the invention. I make no doubt but that ingenious artiſts would ſoon greatly improve this rough ſketch, if the contrivance ſhould be found by experience to anſwer at ſea.

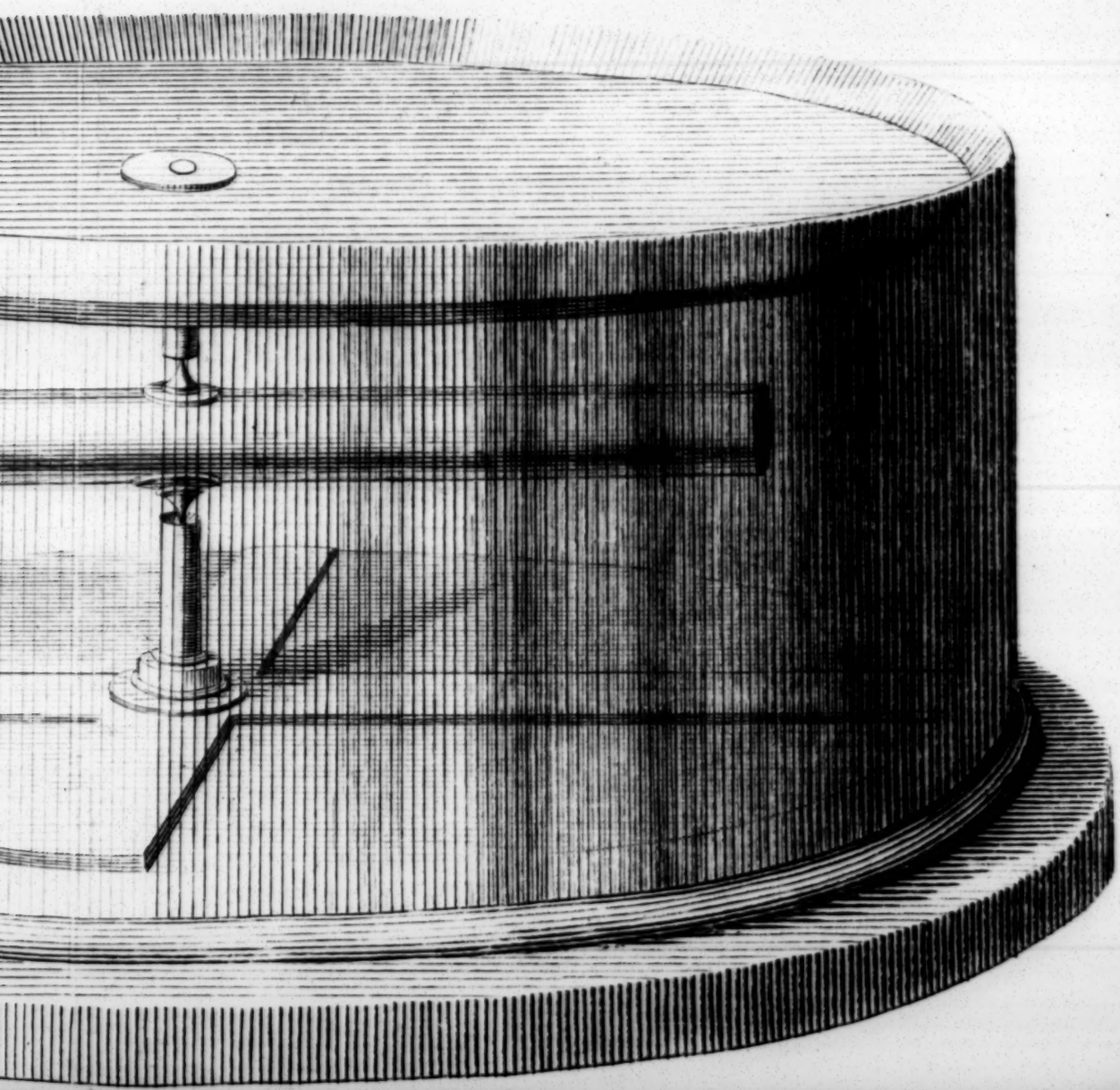












XXXV. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1778. *By* Thomas Barker, *Eſquire. Communicated by* Thomas White, *Eſquire, F. R. S.*

Read May 20, 1779.

		Barometer.			Thermometer.						Rain.
					In the Houſe.			Abroad.			
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low	Mean	
Jan.	Morn.	29,90	28,16	29,27	42½	32	37	41	18½	32	1,980
	Aftern.				43½	32½	38	44½	21	37	
Feb.	Morn.	29,85	28,66	29,33	44½	34	38	45	24	33	0,949
	Aftern.				45	34	39	51	32	38½	
Mar.	Morn.	29,98	28,61	29,32	51	35½	41½	52	24½	36	1,196
	Aftern.				51½	37½	43	59	36	44	
Apr.	Morn.	29,81	28,92	29,31	56	41½	47½	51	30	41	1,037
	Aftern.				58	43	49	65½	40	51	
May	Morn.	29,79	28,94	29,41	61	48½	54	59½	45	50	1,322
	Aftern.				62	49	55½	70	52	61	
June	Morn.	29,86	29,18	29,58	66	53	60	65½	43	55½	2,714
	Aftern.				68	55	61½	80½	57	69	
July	Morn.	29,82	29,02	29,48	70½	59	64½	66	51½	59½	4,103
	Aftern.				73½	61	66½	85	62½	72½	
Aug.	Morn.	30,00	29,10	29,69	67½	56	63	63	42	56	0,391
	Aftern.				70	56	65	76	57	69	
Sept.	Morn.	29,99	28,72	29,54	62½	48	56	55½	32½	46	1,660
	Aftern.				64	50	57	66	44½	58½	
Oct.	Morn.	29,64	28,62	29,31	55	44	48	52	26	39½	4,238
	Aftern.				57	45	49½	61	38½	50	
Nov.	Morn.	29,72	28,63	29,23	52	42	46½	53	30	40½	3,845
	Aftern.				53	43	47	56	38½	46	
Dec.	Morn.	30,23	28,25	29,34	51	40	44	51½	28	40	2,835
	Aftern.				51½	41	45	52½	33	44	
											26,270

This

This ſummer having been hotter than uſual, I here give an abſtract of the hotteſt month in it.

		Barometer.			Thermometer.					
					In the Houſe.			Abroad.		
		Higheſt	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean
June 22 to July 21	Morn.	29,82	29,10	29,58	70½	62½	65½	66	54	61
	Aftern.				73½	63	67½	85	70	75

The year began with froſt. The ſharpeſt and longeſt this winter which laſted about a fortnight; but the ſnow never got deep, and the froſt towards the end was much broken, and the winter was not, on the whole, either ſevere or wet. Some windy wet weather followed the froſt; but it was oftener dark, fair, calm, and cold, and frequently ſcarce either froſt or thaw. The ſpring ſeed time was good; at firſt dark and cold, but a fortnight at the end of March and beginning of April was ſunny, fine, and warm, and ſome days quite hot; then it turned cold again, with ſeveral very ſharp, froſty mornings, and ſometimes hail and ſnow.

Towards the end of April it grew mild and growing again; frequent ſmall ſhowers, and ſometimes windy the firſt half of May. As ſummer advanced it was drier and hotter; very much ſo in June and July, being the hotteſt ſummer ſince 1762, if not ſince 1750. The

ground was much burnt, but not ſo much as ſometimes; for two very heavy thunder ſhowers, June 27 and 28, kept the graſs from entirely failing. But as we had ſcarce a ſettled rainy day for half a year, only ſhowers often with thunder, thoſe places where they did not fall were much more burnt than we, which was the caſe in moſt of the South and Eaſt of England, and I believe to the North and Weſt of us they had more rain than we. Many and heavy ſhowers in the twelve laſt days of July made the graſs grow again for a while; but the harveſt was exceeding fine, not a day's hindrance, many finiſhed in Auguſt, and the crops were in general pretty good. At that time the ground burnt again pretty much, there were ſome ſhowers in September, but the ſeaſon was in general dry and calm, and it was upon the whole a very pleaſant year.

Froſty mornings began early, for there were ſome before September was out; and ſoon after the beginning of October it grew wetter, often windy and frequent froſty mornings. This dark, wet ſeaſon continued till toward the middle of December, and grew more ſtormy, but fewer froſty mornings after October. In ſcarce ten weeks there fell near half a year's rain; the dryneſs of the ground carried it off for a good while, but it was very full of wet at laſt. The wheat ſeed-time being early, and

the clays ſowed before the ground got wet, it went very well into the ground, and looks finely in moſt places. The latter part of December was dark, fair, and mild, and in general calm, till the laſt day a violent ſtorm, which ſome ſuppoſed as ſtrong as that in 1703 did a great deal of damage in the North of England, and the Eaſtern part of the South of it; but does not ſeem to have been ſo ſtrong toward the Weſtern parts.

XXXVI. *Extract of a Meteorological Journal for the Year 1778, kept at* Briſtol, *by* Samuel Farr, *M. D.*

Read May 20, 1779.

Months.	Barometer. Higheſt.	Loweſt.	Mean.	Viciſſitude.
January	30.31	28.54	29.37	1. −2
February	30.34	29.25	29.81	1. −3
March	30.38	29.10	29.77	0.94−1½
April	30.25	29 30	29.78	0.30−1
May	30.28	29.40	29.56	0.30− ½
June	30.34	29.65	30.20	0.23− ½
July	30.29	29.50	29.94	0.24− ½
Auguſt	30.48	29.73	30.15	0.40−1½
September	30.40	29.30	29.80	0.77−1
October	30.10	29.18	29.70	0.50− ½
November	30.26	29.12	29 69	0.60− ½
December	30.74	28.79	29.86	0.86−1½

\+ riſing.
− falling.

Months.	Thermometer in. Higheſt	Loweſt.	Mean.	Viciſſ.	Thermometer out. Higheſt	Loweſt	Mean.	Viciſſ.
				° Days				° Days
January	44	31	36	+ 17–1½	46	30	36	+ 12–1½
February	46	35	40	− 4– ½	48	32	40 8/10	− 8– ½
March	52	39	43	− 9–2½	56	36	44 8/10	− 9– ½
April	63	43	51	+ 6– ½	70	41	53	− 10–1½
May	66	50	57	+ 4– ½	70	48	58	− 12– ½
June	74	55	61	− 5– ½	78	53	64	+ 12– ½
July	75	61	67	+ 4– ½	79	57	68½	− 7–1½
Auguſt	73	59	65	− 7–1½	78	61	68	+ 7–1
Septemb.	64	47	57	− 7– ½	67	46	58	− 12–1
October	57	41	49	+ 11–1½	59	38	47	− 11– ½
Novemb.	55	43	49	− 4– ½	57	39	46	+ 7– ½
December	54	38	45	− 10–1½	53	28	43 8/10	+ 15–1
			51½				52	

An

An abridged table of the WINDS, &c. for BRISTOL, for the Year 1778.

	N	E	W	S	NW	SE	NE	SW	Rain.	Fair Days.	Frosty Days.	Thunder, &c.
Jan.		2		1		9	11½	7½	2.199	7	11	20. 21. 22. S.W.
Feb.	1½	1½	½	½	4½	10	3½	6	0.957	8	6	
Mar.	1	1		2		3½	15	8½	1.406	12	5	
Apr.		½			4½		13	12	0.663	16		30. E.
May	½		½	1½	1½	5	4	18	2.716	20		
June		1		2	1	1	5½	19½	0.972	19		15. N.W. 24. E. 26. S.W. 29. N.E.
July	1			1	2½	1½	3	22	3.003	22		20. N.E. 28. 31. S.W.
Aug.		½		1½	1½	4	8	1 5½	0.292	22	1	
Sept.	½			½	6	5½	11	6½	1.323	20	3	11. S.W. 27. N.E.
Oct.		½	½	1	6	11½	8½	3	3.987	18	11	
Nov.	1½			½	14½	2	1½	10	5.085	12	5	25. N.W.
Dec.	½	1	3	½	9	5	½	11½	3.300	11	6	
	6½	8	4½	12	51	58	85	140	25.903	187	48	

WEATHER FOR THE YEAR 1778.

January. Cloudy but dry till 4th, with a froſt; rain on 4th and 5th; froſty till 12th; then rainy, except 16th, till 26th; then a froſt till 28th, when it thawed as on 29th; 30th and 31ſt wet.

February. Dry to the 4th and on 5th; then wet to 9th; dry and froſt on 13th to 14th; 16th, 17th, 18th, and 19th, froſty; then wet till 26th; that and 27th had ſome ſnow; 28th fair,

March. Fair to the 6th; 7th cloudy; 8th wet; cloudy but dry to the 12th, and dry to the 20th; wet to 25th; dry to 28th; after that wet to the end. April.

April. Dry to the 6th, and after that to 9th, and then to the 13th, and after that to 16th; 17th and 18th fair; 19th and 20th ſtormy; then dry and froſty to 27th; 28th cloudy; 29th and 30th wet.

May. The 1ſt dry; then it was wet to the 9th; dry to the 12; then it rained to 19th; was dry to 22d, and afterwards, except 27th, to the end.

June. Wet, except the 3d, till 6th; 7th wet; 8th dry; 9th wet; then dry to 15th, and after that to 24th; 25th dry; 26th, 27th ſtormy; 28th dry; 29th ſtormy; 30th dry.

July. The 1ſt and 3d wet; 2d dry, and to the 16th; then it was wet the reſt of the month.

Auguſt. Some rain on 3d, 6th, 14th, 15th, and 30th; the reſt was quite dry, and chiefly fair.

September. Was dry to the 7th, and on 8th and 9th; then wet to 13th; dry to 16th; 17th wet; then dry to 24th; 25th dry; 26th and 27th wet; 28th, 29th, a froſt; 30th wet.

October. The 1ſt wet; 2d dry; 3d wet; 4th, 5th, dry; 6th, 7th, 8th, wet; then froſty to 19th; after which, except on the 25th, 26th, 29th, it was very wet.

November. The 2d, 8th, 14th, 15th, 20th, 21ſt, and 30th, were dry; the reſt were all wet.

December. 1ſt and 5th froſty; then it rained till 25th, excepting 19 and 21ſt; then a froſt to 28th; then wet to the end.

XXXVII. *A Treatiſe on Rivers and Canals.* By Theod. Aug. Mann, *Member of the Imperial and Royal Academy of Sciences at* Bruſſels; *communicated by* Joſeph Banks, *Eſq. P. R. S.*

Read June 24, 1779.

TO JOSEPH BANKS, ESQ. P. R. S.

SIR,

YOUR election to the Preſidency of the firſt literary and ſcientific Society in the world, to a chair ſo long and ſo glorioufly occupied by the great NEWTON; joined to the friendſhip you have been pleaſed to honour me with ſince my being firſt known to you: has encouraged me to ſend you ſomething of my compoſition, as the beſt way of expreſſing my ſincere reſpect and attachment to you, and my profound veneration for the illuſtrious Body which has choſen you for its head. Though various circumſtances, by carrying me very early into foreign countries, have made me from my youth almoſt an

an alien to my native ſoil, and put me in a ſituatio. which apparently muſt make me ever remain ſo; yet, neither time nor diſtance could ever weaken, much leſs obliterate, my tender attachment to it, or my ardent wiſhes for its welfare.

Theſe conſiderations will, I hope, merit a favourable acceptance from the Royal Society of the following piece, which I have the honour of addreſſing to you; and an indulgent condeſcenſion for its imperfection in every reſpect, and particularly in point of ſtyle. Five and twenty years abſence from my native country, and the neceſſity of converſing during that time in different foreign languages, muſt unavoidably have filled mine, without my being ſenſible of it, with idioms and expreſſions in no wiſe Engliſh.

As to the ſubject I have undertaken to treat on this occaſion, I was guided in the choice thereof by the motive of ſaying ſomething that might be uſeful to my native country. The great number of extenſive and magnificent canals, which have been cut through almoſt every part of England of late years, for the uſe of internal navigation, and which do honour to the public ſpirit of the nation, merit to be conſidered in a ſcientifical as well as in a commercial light. Their waters have their laws of motion different in many caſes from thoſe of

rivers; they are liable to many accidents which the others are not, and of a different nature. Theſe accidents do not become ſenſible till many years after their conſtruction, and are better prevented in time than remedied when they happen. I have long lived in a country famous for its navigable canals, and have been much employed, under the eyes of the government of it, upon that ſubject. I only mention this to ſhew, that I have not undertaken to treat a ſubject to which I am an utter ſtranger.

There are, moreover, many conſiderations concerning the laws of motion in rivers and canals in general, the velocity of their currents in proportion to the quantity of their declivity, and the means of aſcertaining therefrom the reſpective heights of the interior parts of continents, which merit the attention of a natural philoſopher. I ſhall venture to offer my thoughts and obſervations (ſome of which, I believe, are new) on all theſe ſubjects in the enſuing Diſſertation, which I ſubmit entirely to the judgment of the Royal Society, and ſhall eſteem myſelf happy if I ſucceed in it, ſo as to be of any uſe to my country, and to be able to teſtify, at the ſame time, my profound reſpect and veneration to you, DEAR SIR, and to the illuſtrious body over which you preſide.

SECTION I.

Different uſes for which canals are made, with an account of the principal authors who have wrote concerning them.

1. Artificial canals are to be conſidered in a double light; as facilitating commerce by means of internal navigation, and as preventing inundations by carrying off the too great abundance of water from low and flat countries, ſuch as are Holland, Flanders, &c. In theſe laſt named countries they ſerve at once for both purpoſes; and it is in this double light that I ſhall conſider them in the enſuing diſcourſe. If canals for draining have ſluices upon them, particularly at the end whereby they diſcharge their waters, as is univerſally the caſe in the Low Countries, they differ in no wiſe from navigable canals: if they have nothing to ſuſtain their waters in them, they are to be conſidered in every reſpect as rivers or rivulets, and follow the ſame laws. It muſt, therefore, be carefully kept in mind, that whenever I mention canals, I mean thoſe only whoſe waters are kept up by ſluices, and never thoſe without them, which I include, without diſtinction, under the common appellation of rivers; for they

they are no more than artificial ones. If I miſtake not, all the navigable canals in England are of the firſt ſort; that is, have their waters kept up, and let off by ſluices. This neceſſary diſtinction will take away all ambiguity from what I have to ſay on canals throughout the following diſcourſe.

2. But that I may fulfil the taſk I have undertaken, it is neceſſary firſt of all to lay down ſuch principles on the nature of rivers and canals in general as have been demonſtrated true both by calculation and experience; to the end, that we may deduce from thence the true laws of motion of their waters, and the quantity of declivity of their beds: for this purpoſe, and becauſe a large volume would hardly ſuffice to compriſe all the demonſtrations of theſe principles, which, conſequently, I am obliged to omit in this treatiſe, it will not be amiſs to mention the principal authors who have treated this ſubject in different ages and countries, in whoſe works the demonſtrations of all the principles I ſhall lay down may be found, if any one doubts the truth of them. Theſe are the following.

SEXTUS JULIUS FRONTINUS, de Aquæ-ductibus Urbis Romæ, cum Notis POLENI, impreſſ. 1722.

JOHN BAPTIST ALEOTTI, Hydrometrician to the Duke of Ferrara, and to Pope CLEMENT the VIIIth.

* Don BENEDICT CASTELLI, Benedictine Abbot, de Menſurâ Aquarum Currentium.

J. B. BARATTERI, de Architettura d'Acque, lib. VI. Piacenza, in folio, 1656.

ALEXANDER BELTINZOLI, of Cremona.

NICOLAUS CABEUS, in Libris Meteorum.

GALILEI GALILEO.

* JOH. BAPT. BALIANI, de Motu Liquidorum.

* JOH. BAPT. RICCIOLI, Geographiæ et Hydrographiæ Reform. libro VI. cc. 29. et 30.

* CLAUDE MILLET DESCHALES, de Fontibus et Fluminibus, à prop. 39. uſque ad 56.

VARENNIUS, General Geography, with Dr. JURIN's and Dr. SHAW's Notes, edit. of 1765, vol. I. from page 295 to page 358.

Dr. JURIN, in the Philoſophical Tranſactions, N° 355. page 748. et ſeq.

MARIOTTE, Traité du Mouvement des Eaux.

VARIGNON, Memoires de l'Academie des Sciences de Paris, pour 1699 et 1703.

* Sir ISAAC NEWTON, Princip. Mathem. lib. II. § 7. page 318 et ſeq. edit. 1726.

* DANIELIS BERNOUILLI, Hydrodynamica, in quarto, Argentorati, 1738.

* DOMENICHE GUGLIELMINI, della Natura de Fiumi, Bononiæ, 1697, in quarto. Ejuſdem de Menſurâ Aquarum fluentium, Bononiæ, in quarto.

* JOH. POLENUS, de Caſtellis et de Motu Aquæ mixto, Patavii, 1697, 1718, 1723.

* RACCOLTA d'Autori che trattano del Moto dell' Acque, Fiorenza, 1723, 3 vol. quarto, cum fig.

JAC. HERMANNUS, in Phoronomia, cap 10. page 226. et ſeq.

CHRIST. WOLF, Curſ. Mathem. Hydraulicæ, cap. VI. edit. Genevæ, in quarto, 1740.

M. DE BUFFON, ſur les Fleuves, dans ſon Hiſtoire Naturelle, tom. II. p. 38—100. de la 1ere edit. en 12mo.

Several Memoirs upon this Subject in the Collection of the Royal Academy of Sciences of Paris, particularly thoſe of M. PITOT, in the volumes for 1730 and 1732.

s'GRAVESANDE, in Elementis Phyſicæ, tom. I. lib. II. cap. 10.

* R. P. LECCHI S. J. Hydroſtatica, Mediolani, 1765. In this excellent work are ſeveral pieces by Father BOSCOVICH upon the ſame ſubject.

* STATTLERI Phyſica, vol. III. p. 232—286. de curſu Fluminum, ejuſque Menſuratione et Directione. Aug. Vindel. 1772, 8 vol. in octavo. This author gives many late obſervations and experiments on the motion and meaſure-

meaſure of currents, as thoſe of ZENDRINI, HIMENII, &c.

Two other authors have lately wrote upon rivers and canals, but their works are not yet come to my hands; to wit,

Father FRISI, an Italian Barnabite, Profeſſor of Mathematics at Milan.

M. DE LA LANDE, of the Royal Academy of Sciences at Paris, who has juſt publiſhed a Hiſtory in folio, with plates, of all the Navigable Canals in the World that have come to his Knowledge.

Among the above authors, thoſe marked with an aſteriſk (*) are they who have treated the ſubject in queſtion with the greateſt exactneſs or moſt extent; and it is from them chiefly that I ſhall lay down ſuch principles and laws of action in rivers and canals as regard the ſubject I have taken in hand. By this means I ſhall avoid advancing any thing upon ſo important a matter, but what is founded upon the moſt certain and exact experiments, and conformable to what are demonſtrated to be the real and unalterable laws of nature.

SECTION II.

The theory of rivers and canals.

I. DEFINITIONS.

3. A river is a greater or leſſer quantity of water which runs conſtantly, by its own gravity, from the more elevated parts of the earth, towards thoſe which are more depreſſed, in a natural bed or channel open above.

4. If this bed or channel is artificial, and has been dug by hands, it is called *a canal*, of which there are two kinds; thoſe where the channel is every where open, and without ſluices, which I call an *artificial river*; and thoſe where the waters are kept up or let off by the means of ſluices: it is this ſecond ſort which I ſhall call henceforwards by the proper name of *a canal*.

5. *A river is ſaid to perſevere in the ſame ſtate* ſo long as there runs off an equal quantity of water in the ſame time, without any increaſe or diminution, ſo that it remains always at the ſame height in the ſame place. When the circumſtances are different from this, it is ſaid reſpectively that *a river increaſes or diminiſhes*.

6. *A ſection* of the bed of a river or canal is a plane drawn perpendicular to the bottom of the bed and to the

direction of the ſtream of water, and whoſe limits are thoſe of the water itſelf which runs off in that place.

7. I call *ſections of equal velocity*, all thoſe where the water runs with equal velocity; and *ſections of greater or leſſer velocity*, thoſe where the water runs faſter or ſlower reſpectively, and when compared to others.

8. I call *mean velocity of a current or ſtream of water*, that which a river or canal would have, if all the parts thereof, to wit, thoſe of the bottom, the ſides, the middle, and the ſurface of the ſame ſection, ran with an equal velocity, in ſuch a manner that there would paſs juſt as much water in the ſame time by this uniform motion, as there does now actually paſs by the irregular flowing of the ſtream.

II. PROPOSITIONS, *or laws of action in rivers and canals.*

9. The motion of water in rivers proceeds from the ſame principle which produces the deſcent of heavy bodies upon inclined planes.

11. The deſcent of heavy bodies upon inclined planes follows exactly the ſame laws as thoſe obſerved in the deſcent of heavy bodies in a perpendicular line towards the center of the earth; that is,

1ſt, They deſcend by a motion uniformly accelerated.

2dly, The

2dly, The ſpaces, run over by heavy bodies which fall perpendicularly by a motion uniformly accelerated, are in *a duplicate ratio of the times and velocities* reſpectively.

3dly, Theſe ſpaces, in equal times, augment in the ſame ratio as the odd numbers in progreſſion 1, 3, 5, 7, 9, 11, &c.

4thly, Therefore, both the times and the velocities are in a *ſub-duplicate ratio of the ſpaces run over.*

5thly, It is demonſtrated by the principles of mechanics, that the velocity acquired by a heavy body deſcending freely upon an inclined plane, in a given time, is to the velocity which the body would acquire in the ſame time, by falling perpendicularly, as the height of the inclined plane is to its length.

6thly, From whence it follows, that the velocities which bodies acquire in their deſcent upon inclined planes, are *in a direct ratio of the ſquare roots of the quantity of inclination or declivity of the planes.*

11. So that when water flows freely upon an inclined bed, it acquires a velocity, which is always as the ſquare root of the quantity of declivity of the bed.

12. In an horizontal bed, opened by ſluices or otherwiſe, at one or both ends, the water flows out by its gravity alone; and the flowing is quicker or ſlower in a di-

rect ratio of the reſpective heights of the water, by reaſon of the weight of the ſuperior waters upon the inferior.

13. From hence (N° 11, 12.) it follows, firſt, that as much as the declivity of the bed or channel of a river is greater, ſo much alſo will the velocity of the flowing waters be proportionably increaſed.

2dly, As much as the water in an horizontal bed is deeper, ſo much will the velocity of the current be increaſed; and this velocity will diminiſh in proportion to the decreaſing depths of the water in the bed.

3dly, Abſtracting from the reſiſtance cauſed by the bottom and ſides of the bed, as much nearer as the water is to the bottom, ſo much will its motion be accelerated; not only becauſe the inferior waters are more compreſſed by the ſuperior in proportion to their greater depth; but alſo becauſe the inferior ones have a greater declivity than the ſuperior, by reaſon of their greater depth in the bed, where they are more depreſſed with reſpect to the elevation of their common ſource or ſpring. But theſe different velocities of the upper and lower waters in the ſame ſection of the bed (abſtracting from the friction of the bottom and ſides) approximate indefinitely to each other in proportion to the length of the channel, but ſtill without a poſſibility of their ever becoming equal in fact, if they met with no reſiſtance from the bed.

14. There-

14. Therefore, the motion of water flowing freely in an inclined channel, is accelerated by its own weight combined with the quantity of declivity in the bed.

Nevertheleſs, the velocity of waters which flow in an inclined bed, during their actual flowing, is not accelerated by the weight which the inferior waters ſuſtain from the ſuperior ones, in caſe the lower parts have already, by the declivity of the bed, a greater velocity than that which the weight of the ſuperior ones impreſſes upon them. The reaſon of which is, that no body which follows another with a leſſer velocity can act by impulſion upon that which precedes it with a greater velocity, as is the caſe with regard to theſe ſuperior and inferior waters. But the weight of the upper waters begins to accelerate the lower as ſoon as they fall into an horizontal bed, or one that is ſo nearly horizontal as to deſtroy the greater velocity of the lower waters above that of the upper.

15. The velocity of rivers depends ſometimes upon the ſole declivity of their beds; ſometimes alſo upon the ſole gravity of their waters: and if theſe two cauſes ſometimes act together, the effect produced is only the reſpective exceſs of the one above the other. It often happens in the ſame ſection of a river, that the acceleration of velocity in the inferior parts proceeds from the weight of

the upper waters, while that in the upper parts proceeds from the declivity of the bed.

From whence it follows, that in rivers which have little declivity, it is the depth of the waters which contributes moſt to accelerate their current; and in thoſe whoſe beds have moſt declivity, it is the deſcent of gravity upon an inclined plane which has the greateſt ſhare in producing this acceleration.

To find whether the water in a part of a river where the bed is nearly horizontal flows by the velocity acquired in the preceding declivities, or by the compreſſion of the upper waters upon the lower in that place; a pole muſt be thruſt down to the bottom, and held perpendicular to the current of the water, with its upper end above the ſurface: if the water ſwells and riſes immediately againſt the pole, it ſhews that its flowing is by virtue of a preceding declivity: if, on the contrary, the water ſtops for ſome moments before it begins to riſe againſt the pole, it is a proof that it flows by means of the compreſſion of the upper waters upon the lower.

16. The *abſolute height or elevation* of the ſurface of a river which perſeveres in the ſame ſtate (N° 5.) continually decreaſes, as the diſtance in the river from its ſource increaſes; by reaſon that its bed muſt continually incline and tend towards the center of the earth.

17. The

17. The velocity of each particle of water in a regular channel, that is, where the bed is a regularly inclined plane, may be determined by drawing a perpendicular from the particle propoſed to the horizontal curve which paſſes through the ſpring, or that point of the river where the particle in queſtion begins to acquire its velocity. For the velocity which this particle would acquire, in falling freely along the ſaid perpendicular, is the ſame as that which it has acquired in its deſcent along the inclined plane of its bed.

18. So long as *a river perſeveres in the ſame ſtate* (N° 5.) there flows an equal quantity of water in equal times, how unequal ſoever the ſections be through which they flow; and, conſequently, where the ſection of the river is greater, the velocity of the flowing water is leſs; and where the ſection is leſs, the velocity is greater; always in an inverſe proportion. From hence may be deduced the following and other ſimilar propoſitions.

1ſt, Through equal ſections, in equal times, and with equal velocity, there muſt flow equal quantities of water.

2dly, Through equal ſections, in equal times, but with unequal velocities, the quantities of water which flow, are in a direct ratio of the reſpective velocities.

3dly, Through

3dly, Through unequal ſections, in equal times, and with equal velocities, the quantities of water which paſs are in a direct ratio of their reſpective ſections.

4thly, Through unequal ſections, and with unequal velocities, the quantities of water which flow in equal times, are in a combined ratio of the ſections and mean velocities (N° 8.) together.

In a word, the ſections of the bed, the mean velocities, the times of flowing, and the quantity which flows, are univerſally in a combined ratio together; and this combination is what is called *the momentum of a river*; and this *momentum of the ſame flowing water is univerſally equal.*

19. From hence may eaſily be deduced the principles for calculating the quantity of diminution of the water in a lake, pond, or veſſel, by any determinate flowings whatſoever: for as the ſurface of the lake, &c. is to the ſection of the current which carries off the waters; ſo is the mean velocity of the current in this ſection to the decreaſe of the waters in the lake, &c. and *vice verſâ.*

III. *On the nature of rivers and flowing waters.*

20. Rivers contain divers inherent cauſes of the acceleration of their motion.

1ſt, Their ſprings are either in mountains or on high grounds, and it is by the deſcent of the waters from theſe elevations that they acquire a velocity and acceleration of motion ſufficient to ſuſtain and propagate it through the reſt of their courſe.

2dly, The coheſion of the particles of a fluid, *in a bed ever ſo little inclined*, is a ſecond cauſe of acceleration of motion in the fluid; becauſe, by their mutual attraction, thoſe particles which begin firſt to flow draw after them thoſe which are contiguous, theſe the following, and ſo on *ad infinitum*.

3dly, Moreover, where a river, by flowing in a bed nearly horizontal, has loſt a great part of the velocity which it had acquired in the preceding declivities, and the bed by this means is become large and ſhallow, which conſequently again augments the ſlowneſs of the current; it may, however, recover a part of its velocity, even in the ſame horizontal bed, by augmenting the depth thereof, and diminiſhing its breadth; for by this means the weight of the ſuperior waters upon the infe-

rior is increaſed, and conſequently the velocity of the whole is augmented (N° 13. 18.). In the ſame manner a junction of rivers in the ſame bed, by excavating and deepening it, augment the velocity of the common current, as we ſhall ſhew more particularly hereafter.

21. On the other hand, flowing waters meet with many powerful cauſes of reſiſtance to their motion, which tend continually to diminiſh their velocity. Such are the following:

1ſt, The attraction and continual friction of the bottom and the ſides of the bed, contribute greatly towards retarding the motion of the water.

2dly, The ſame effect is produced likewiſe by the many obſtacles which they meet with in their way; ſuch as inequalities in the bottom and ſides of the channel, banks of ſand and mud, rocks, trunks of trees, and other ſuch things.

3dly, The many windings and angles made in their courſe, which produce ſo much the more reſiſtance and hindrance to the motion of the water, as the courſe varies more and oftener from a right-line.

4thly, The diminution of their declivity the farther they recede from their ſprings; this being generally the leaſt towards their mouths, which are for the moſt part in extenſive plains.

Finally,

Finally, the natural cohesion of the particles of water *in an horizontal bed* contributes to retard its motion precisely by the same force which contributes to accelerate it *in an inclined bed.* By diminishing or taking away the above obstacles to the free motion of water in rivers and canals, the velocity of their currents will be increased in the same proportion, and thereby also all the dangers and ravages of inundations may be prevented, as we shall shew hereafter.

But if some or all of these causes, in a greater or lesser degree, did not exist in rivers of considerable depth and declivity, it is demonstrated (a), that the velocity of their currents would be accelerated to twelve, fifteen, and, in some cases, even to twenty times more than it is at present in the same rivers, whereby they would become absolutely unnavigable.

22. The waters in a river or open canal have their motion accelerated, so long as the effects proceeding from gravitation, declivity, depth, in a word, so long as *the sum of accelerations surpasses the sum of resistances.*

When these different sums become equal to each other, the motion of the water is neither accelerated nor retarded, but remains equal, till something anew destroys the *equilibrium.*

(a) Vide LECCHI, Hydrostat. et STATTLERI, Physic. tom. III. p. 252.

When the ſum of reſiſtances and cauſes of retardation is greater than the ſum of accelerating cauſes, the velocity of the river is diminiſhed in proportion to the excess.

23. The *percuſſion* of the waters of a river againſt an obſtacle which is oppoſed to their motion, is the action of the waters ſtriking againſt that obſtacle; and the principles for calculating the quantity of this percuſſion, or the effects which any obſtacles whatever produce in the motion of rivers, by known forces, and in determinate times, are as follows:

1ſt, The percuſſion of the water of a river againſt any obſtacle whatever is univerſally in a compound ratio of the quantity of the plane or planes which the obſtacle oppoſes to the current of water, of the ſine of the angle of incidence which the direction of the current makes with theſe planes, and of the ſquare of the velocity of the ſaid current.

2dly, The reſiſtance, therefore, which the bed of a river oppoſes to its current from any particular obſtacles in it, is in a compound ratio of the magnitude and ſituation of the planes of theſe obſtacles, together with the ſquare of the current's velocity in the place where thoſe obſtacles are found.

3dly, The accelerating force of a river, or that by which it ſurmounts the reſiſtance of its bed in any one

place, compared to that in another, is in a compound ratio of the mass of water and of the velocity of the current in those places respectively.

24. In different parts of the same river, the velocity of the current is greater in a direct proportion of the greater declivity of the bed; because the relative gravity of the flowing particles augments in that ratio.

25. But in the same section of a river, the superior parts, and those which are farthest from the bottom and the sides, will continue their course by the sole cause of the declivity of their bed, *how little soever it be*; because these waters not being retarded by the friction of the bottom and sides of the bed, or hardly by any other obstacle whatever, *the least possible deviation from a level will produce a current.* But the waters at the bottom of a river, both because of their friction against it, and of the irregularities which are almost every where found in it, will lose that little motion which a very small declivity can give them, and their motion in that case will be produced alone by the compression of the superior waters upon them.

The inferior waters which thus acquire their motion from the weight of the superior ones upon them, communicate reciprocally a part of their motion, by means of the natural cohesion of the particles together (N° 20.)

to the ſuperior ones, which in an horizontal bed, without this cauſe, would have no other motion than that which is impreſſed upon them by the impulſive force of the waters deſcending from their elevated ſprings.

From whence it appears, that the ſuperior and inferior waters in a river communicate reciprocally a part of their motion to each other; but this can never go beyond a certain point or *maximum*, which is always proportionable to the *momentum* of the river in that place (N° 18.).

It follows from hence, that the greateſt velocity of a river, running in a right-line, is in *the center of its ſection* (N° 6.); that is to ſay, in that point which is the fartheſt poſſible from the ſurface of the water and from the bottom and ſides of the bed, all taken together. This part has the advantage of one half of the depth of water preſſing upon it, and it is exempt from the friction of the bottom and ſides of the bed which are there overcome and vaniſh by the perpendicular compreſſion.

On the contrary, the leaſt velocity of the water is at the bottom and ſides of the bed, becauſe it is there that the reſiſtance produced by friction is greateſt, from whence it is communicated to the other parts of the ſection in *an inverſe duplicate proportion of the diſtances from the bottom and ſides combined together*, until it becomes a negative

negative quantity, where the effect vaniſhes, or is reduced to nothing.

26. The beſt and moſt ſimple method of meaſuring the velocity of the current of a river or open canal, that I know of, is the following:

Take a cylindrical piece of dry, light wood, and of a length ſomething leſs than the depth of the water in the river: round one end of it let there be ſuſpended as many ſmall weights as may be neceſſary to keep up the cylinder in a perpendicular ſituation in the water, and in ſuch a manner that the other end of it may juſt appear above the ſurface of the water. Fix to the center of that end which appears above water a ſmall and ſtraight rod, preciſely in the direction of the cylinder's axis; to the end, that when the inſtrument is ſuſpended in the water, the deviations of the rod from a perpendicularity to the ſurface of it may indicate which end of the cylinder advances the faſteſt, whereby may be diſcovered the different velocities of the water at different depths; for if the rod inclines forwards according to the direction of the current, it is a proof that the ſurface of the water has the greateſt velocity; but if it inclines back, it ſhews that the ſwifteſt current is at the bottom; if it remains perpendicular, it is a ſign that the velocities at the ſurface and bottom are equal.

This

This inſtrument being placed in the current of a river or canal receives all the percuſſions of the water throughout the whole depth, and will have an equal velocity with that of the whole current *from the ſurface to the bottom* at the place where it is put in, and by that means may be found, both with eaſe and exactneſs, the mean velocity of that part of the river for any determinate diſtance and time.

But to obtain the mean velocity of the whole ſection of the river, the inſtrument muſt be put ſucceſſively both in the middle and towards the ſides, becauſe the velocities at thoſe places are often very different from each other. Having by this means found the *difference of time required for the currents to run over an equal ſpace*; or, *the different diſtances run over in equal times*, *the mean proportional* of all theſe trials, which is found by dividing the common ſum of them all by the number of trials *will be the mean velocity of the river or canal.*

If it be required to find the velocity of the current only at the ſurface, or at the middle, or at the bottom, a ſphere of wood, of ſuch a weight as will remain ſuſpended in equilibrium with the water at the ſurface or depth which we want to meaſure, will be better for the purpoſe than a cylinder, becauſe it is only affected by the

water

water of that ſole part of the current where it remains ſuſpended.

It is very eaſy to guide both the cylinder and the globe in that part which we want to meaſure, by means of two threads or ſmall cords, which two perſons muſt hold and direct, one on each ſide the river; taking care at the ſame time neither to retard nor accelerate the motion of the inſtrument.

Several other methods have been invented for determining the velocity of the currents of rivers and canals, which may be ſeen in moſt of the authors enumerated in the beginning of this eſſay (N° 2.)

V. *Application of the preceding laws of the acceleration and retardation of currents to rivers and canals in general, from whence are deduced the various means of preventing or remedying the defects and inconveniencies which muſt neceſſarily happen to them in a ſeries of years.*

27. By combining together all we have ſaid hitherto upon the nature and theory of motion in rivers, and particularly in the articles 13. 18. 20. 21. and 23. it follows evidently, that *the deeper the waters are in their bed in proportion to its breadth, the more their motion is accelerated;*

celerated; ſo that *their velocity increaſes in an inverſe ratio of the breadth of the bed, and alſo of the greatneſs of the ſection*; from whence are deduced the two following univerſal practical rules:

1ſt, To augment the velocity of water in a river or canal, without augmenting the declivity of the bed, we muſt *increaſe the depth and diminiſh the breadth of its bed.*

2dly, But to diminiſh the velocity of water in a river or canal, we muſt, on the contrary, *increaſe the breadth and diminiſh the depth of its bed.*

The above propoſition is perfectly conformable to obſervation and experience; for it is conſtantly ſeen, that the current is the ſwifteſt where the waters are deepeſt and the breadth of the bed the leaſt; and that they flow ſloweſt where their depth is the leaſt and the breadth of the bed the greateſt. "The velocity of waters," ſays M. DE BUFFON [b], "augments in the ſame proportion as "the ſection of the channel through which they paſs "diminiſhes, *the force of impulſion from the back-waters "being ſuppoſed always the ſame.* Nothing," continues he, "produces ſo great a diminution in the ſwiftneſs of "a current as its growing ſhallow; and, on the contrary, "the increaſe of the volume of water augments its

(b) Hiſt. Nat. tom. II. p. 53. 60. edit. in 12mo.

"velocity more than any other cauſe whatever." The celebrated WOLF, in his Hydraulics[c], aſſures us, that "it is a conſtant and univerſal practice, for accelerating "the current of waters, to deepen the bed, and at the "ſame time to render it narrower."

28. When the velocity which a river has acquired by the elevation of its ſprings and the impulſe of the back-water, is at laſt totally deſtroyed by the different cauſes of reſiſtance which we have enumerated above (N° 21.) becoming equal or greater than the firſt, the bed and current at the ſame time being exactly horizontal, nothing elſe remains to propagate the motion, except *the ſole perpendicular compreſſion of the upper waters upon the lower, which is always in a direct ratio of their depth.* But this neceſſary reſource, this remaining cauſe of motion in rivers, augments in proportion as all the other diminiſh, and as the want of it increaſes: for as the waters of rivers in extenſive plains loſe the acceleration of motion acquired in their deſcent from their ſprings, their quantity accumulates in the ſame bed by the junction of ſeveral ſtreams together, and their depth increaſes in conſequence thereof. This junction and ſucceſſive accumulation of many ſtreams in the ſame bed, which we ſee univerſally in a greater or leſs degree in all rivers throughout the known world, and which is ſo abſolutely

(c) N. 224.

neceſſary to the motion of their waters, can only be attributed, ſays Signor GUGLIELMINI [d], to the infinite wiſdom of the ſupreme Author of Nature.

29. The velocity of flowing waters is very far from being in proportion to the quantity of declivity in their bed: if it was, a river whoſe declivity is uniform and double to that of another, ought only to run with double the ſwiftneſs when compared to it; but in effect it is found to have a much greater, and its rapidity, inſtead of being only double, will be triple, quadruple, and ſometimes even more: for its velocity depends much more on the quantity and depth of the water, and on the compreſſion of the upper waters on the lower, than on the declivity of the bed. Conſequently, whenever the bed of a river or canal is to be dug, the declivity muſt not be diſtributed equally throughout the whole length; but, to give a ſwifter current to the water, the declivity muſt be made much greater in the beginning of its courſe than towards the end where it diſembogues itſelf, and where the declivity muſt be almoſt inſenſible, as we ſee is the caſe in all natural rivers; for when they approach near the ſea their declivity is little or nothing, yet they flow with a rapidity which is ſo much greater, as they contain a greater volume of water; ſo that in

(d) Della Natura de Fiumi.

great

great rivers, although a large extent of their bed next the ſea ſhould be abſolutely horizontal, and without any declivity at all, yet their waters do not ceaſe to flow, and to flow even with great rapidity, both from the impulſion of the back waters, and from the compreſſion of the upper waters upon the lower in the ſame ſection.

30. Whoever is well acquainted with the principles of the higher geometry, will eaſily perceive that it would be no difficult matter ſo to dig the bed of a canal or river, that *the velocity of the current ſhould be every where equal.* It would be only giving it the form of a curve along which a moving body ſhould recede from a given point, and *deſcribe ſpaces every where proportional to the times*, allowance being made therein for the quantity of effect of the compreſſion of the upper waters upon the lower. This curve is what is called the *Horizontal Iſochronic*, being the flatteſt of an infinity of others which would equally anſwer the problem where fluids were not concerned. Upon theſe curves may be ſeen LEIBNITZ, HUYGHENS, and the two BERNOUILLI's, who were the firſt that determined and analyſed them, and alſo many ſucceeding geometricians, if any one is deſirous to occupy himſelf in ſuch ſpeculations as are more curious than uſeful, which is not my purpoſe in this treatiſe.

31. Notwithſtanding all we have ſaid concerning the neceſſity of augmenting the depth of a river in a greater proportion than its breadth, if we would accelerate its current; yet it is certain, that this can only be done to a certain point, without deſtroying that equilibrium which ought to reign between the depth and the breadth of the ſection of the ſtream, and thereby putting the river into a ſtate of continual violence, which will inceſſantly exert itſelf to the deſtruction of the banks and wiers made to keep it in, and that action will always exert itſelf in a direct ratio of the greater or leſs want of equilibrium, as it would be eaſy to demonſtrate by the principles of hydraulics. Theſe ſame principles give likewiſe the juſt proportions of this equilibrium between the perpendicular and lateral compreſſion of the water in any river or canal whatſoever, which vary in an inverſe proportion, according to the different degrees of the declivity and velocity of the current; and in a direct one of the greater or leſs coherence and hardneſs of the ſubſtances which compoſe the bed. Rivers which flow in beds compoſed of homogeneous matter of little conſiſtency, ſuch as ſand, &c. are always more broad than deep, when compared to thoſe which run in beds of matter of greater tenacity. It is manifeſt, that the equilibrium here ſpoken of is real,

becauſe rivers remaining in the ſame ſtate only widen their beds to a certain pitch which they do not ſurpaſs.

32. M. DE BUFFON remarks, "That people accuſ-
"tomed to rivers can eaſily foretell when there is going
"to be a ſudden increaſe of water in the bed from floods
"produced by ſudden falls of rain in the higher countries
"through which the rivers paſs. This they perceive by
"a particular motion in the water, which they expreſs
"in their dialect, by ſaying that *the river's bottom moves*;
"that is, the water at the bottom of a channel runs off
"faſter than uſual; and this increaſe of motion at the
"bottom of the river always announces a ſudden in-
"creaſe of water coming down the ſtream. Nor does
"their opinion therein," continues the ſame author,
"ſeem to be ill-grounded on the nature of things; for
"the motion and weight of the waters coming down,
"though not yet arrived, muſt act upon the waters in
"the lower parts of the river, and communicate by im-
"pulſion part of their motion thereto; ſince a canal or
"river contained in its bed is to be conſidered in ſome
"degree as a column of water contained in a long tube,
"where the motion is communicated at once throughout
"the whole length." In a river or canal, open above, it is only communicated to a certain diſtance; that is, as far as the impulſive force of the new increaſe and ſuperior

rapidity

rapidity of the back-waters acts upon the ſtream, which will always be as far as till this force is gradually, and at laſt wholly, deſtroyed by the ſuperior gravitation of the ſuper-incumbent waters in the ſtream. Something of the ſame kind happens when a very great additional weight comes ſuddenly upon the ſurface of a river or canal; for inſtance, by the launching of a ſhip or of ſeveral boats together upon it. Theſe cauſes increaſe the velocity of the water in the lower parts of the bed, and moreover retard its motion at the ſurface, which effect may properly be called *making the river's bottom move.* For the ſame reaſon, the increaſe of weight of the waters in a ſudden flood, as well as the increaſe of their impulſive force, muſt contribute to produce this effect, and, by increaſing the motion in the bottom of the river, may hinder, for ſome ſpace of time, the ſtream from ſenſibly riſing in the bed.

33. All obſtacles whatever in the bed of a river or canal, ſuch as rocks, trunks of trees, banks of ſand and mud, &c. muſt neceſſarily hinder proportionably the free running off of the water; for it is evident, from what we have ſaid, that the waters ſo far back from theſe obſtacles, until the horizontal level of the bottom of the bed becomes higher than the top of the obſtacles, muſt be intirely kept up and hindered from running off in pro-

portion

portion thereto (N° 23.). Now as the waters muſt continue to come down from their ſources, if their free running off is hindered by any obſtacles whatever, their relative height back from them muſt neceſſarily be increaſed until their elevation, combined with the velocity of their current proceeding from it, be arrived to ſuch a pitch at the point where the obſtacles exiſt, as to counterbalance the quantity of oppoſition or impediment proceeding from thence, which frequently does not happen until all the lower parts of the country round about are laid under water.

34. Now it is certain from all experience, that the beds of rivers and canals in general are ſubject to ſome or others of the obſtacles above mentioned. If rocks or trees do not bar their channels, at leaſt the quantity of ſand, earth, and mud, which their ſtreams never fail to bring down, particularly in floods, and which are unequally depoſed according to the various windings and degrees of ſwiftneſs in the current, muſt unavoidably, in courſe of time, fill up, in part, different places in the channel, and thereby hinder the free running off of the back waters. This is certainly the caſe, more or leſs, in all rivers, and in all canals of long ſtanding, as is notorious to all thoſe well acquainted with them. Hence, if theſe accidents are not carefully, and with a

conſtant

conſtant attention prevented, come inundations, which ſometimes lay waſte whole diſtricts, and ruin the fineſt tracts of ground, by covering them with ſand: hence rivers become unnavigable, and canals uſeleſs, for the purpoſes for which they were conſtructed. Canals, in particular, by reaſon that their waters for the moſt part remain ſtagnant in them, are ſtill more liable than rivers to have their beds fill up by the ſubſiding of mud, and that eſpecially for ſome diſtance above each of their ſluices; inſomuch, that if continual care is not taken to prevent it, or remedy it as often as it happens, they will ſoon become incapable of receiving and paſſing the ſame veſſels as formerly. Nay, the very ſluices themſelves, if the floors of their bottoms are not of a depth conformable to the bed of the canal, will produce the ſame accidents as thoſe we have been ſpeaking of; for if they are placed too low, they will be continually filling up with ſand or mud; if too high, they have the ſame effect as banks or bars in the bed of a river, that is, they hinder all the back-waters under their level from running off, and ſoon fill up the bed to that height by the ſubſiding of mud. This effect is much accelerated by the ſhutting of the lower ſluices, which makes a great volume of water reflow back to thoſe next above them, till the whole is filled and becomes ſtagnant. Now it is evident,

that

that this ſtate of things muſt contribute far more to the ſubſidency of mud and all other matters brought down by the waters in canals, than can be the caſe in rivers whoſe currents conſtantly flow.

35. I do not ſuppoſe that theſe inconveniencies can have yet manifeſted themſelves by any very ſenſible effects in the many new canals and ſluices lately conſtructed in England; but as the ſame cauſes do not ceaſe to act more or leſs every where, the effects which neceſſarily follow from them will likewiſe become more and more ſenſible, unleſs continual care be taken to prevent them. The waters of all rivers and canals are from time to time muddy: their ſtreams, particularly during rains and floods, carry along with them earth and other ſubſtances which ſubſide in thoſe places where their currents are the leaſt, whereby their beds are continually raiſed: ſo that the ſucceſſive increaſe of inundations in rivers, and of unfitneſs for navigation in canals, when they are neglected and left to themſelves, is a natural and neceſſary conſequence of the ſtate of things, which no intelligent perſon can be at a loſs to account for; and yet I have known whole countries remain in this habitual ſtate of negligence to their very great detriment.

36. Having thus ſhewn the principal accidents which rivers and canals are liable to, with the cauſes of them, I

ſhall proceed to point out the moſt efficacious methods of preventing them, or at leaſt of diminiſhing their effects. Perhaps it would have been more proper to have deferred doing this till I ſhould have ſaid all I have to ſay upon the nature of rivers and canals: however, I ſhall forego the more ſcientific order of things, for the ſake of bringing the means of remedying the accidents and inconveniences which happen, nearer to the cauſes that produce them, whereby their connexion and efficacity may be better judged of. For this end, I ſhall here lay down, briefly and in general terms, the methods moſt proper for the purpoſe in queſtion. They flow immediately from the principles already laid down in this eſſay, and do not need many words to make them compleatly underſtood.

37. A work of this kind, if it is properly conducted, muſt be begun at the lower end of the river or canal; that is to ſay, at that end where their waters are diſcharged into the ſea, or where they fall into ſome other greater river or canal, from whence their waters are carried off without farther hindrance. If it is a river whoſe bed, by being filled up with mud, ſand, or other obſtacles, and by being otherwiſe become irregular in its courſe, is thereby often ſubject to inundations, and incapable of internal navigation, the point, from which

the work muſt be begun and directed throughout all the reſt of the channel, is from the loweſt water-mark of ſpring tides on the ſhore at the mouth of the river; or even ſomething below it, if it can be done; though this part will ſoon fill up again by the ſand, mud, &c. which the tides ceaſe not to roll in.

If it is a canal whoſe bed is be to dug anew, or one already made, which is to be cleaned and deepened from the ſea ſhore or ſome large river back into the country, and where no declivity is to be loſt; as is the caſe in all flat countries: the work muſt be begun, and the depth of the whole channel directed, from the low water-mark of ſpring tides, if the mouth is to the ſea, or from ſuch a depth in the channel of the river, if the canal falls into one, that there may be ſuch a communication of water from the canal to the river, in all ſituations of the current, as may let boats freely paſs from one to the other. This, of courſe, muſt alſo direct the depth of the floor of the laſt ſluice towards the mouth of the canal, be it to the ſea or into a river. If the bottom or floor of a ſluice already conſtructed be too low, it will ſoon fill up with ſand or mud, and thereby hinder the gates from opening, unleſs it be continually cleaned out; if, on the contrary, this floor be too high, and in a canal whoſe natural declivity is too little for the free current of the water, as is

generally the caſe in Holland and Flanders, all depth of the bed of the canal below the horizontal level of the bottom of the ſluice will ſerve to no manner of purpoſe, either for navigation, or for carrying off the back-waters, but will ſoon fill up with mud, in ſpite of all means uſed to the contrary, except that of digging it continually anew to no manner of purpoſe; as is evident from the reaſons given above (N° 33. 34.).

38. Setting off from this determinate point, at the mouth of a river, or at the bottom of the laſt ſluice upon a canal, which are to be cleaned and deepened; the work muſt be carried on, in conſequence, uniformly throughout their whole courſe backwards into the country as far as is found neceſſary for the purpoſes intended. This is to be done after the following manner:

1ſt, One muſt dig up and carry away all irregularities in the bottom and ſides of the bed, ſuch as banks of ſand and mud, rocks, ſtumps or trunks of trees, and whatever elſe may cauſe an obſtacle to the regular motion of the water, and to the free paſſage of veſſels upon it.

2dly, If the declivity of the bed ſhould be ſtill too little to give a ſufficient current to carry off the water as often and as faſt as is neceſſary, the whole bed itſelf muſt be regularly deepened, and what is dug out from the bottom muſt be laid upon the ſides, to render it narrower in

proportion

proportion to its depth. The reaſon of this is evident from all that has been ſaid.

3dly, Wherever the banks are too low to contain the ſtream in all its ſituations, they muſt be ſufficiently raiſed; which may be conveniently done with what is dug out from the bed: and the whole being covered with green turf will render theſe banks firm and ſolid againſt the corroſion of the water. It is proper at all times to lay upon the banks what is dug from the bed, by which they are continually ſtrengthened againſt the force of the current.

4thly, It is often neceſſary to diminiſh the windings and ſinuoſities in the channel as much as poſſible, by making new cuts whereby its courſe may approach towards a right line. This is a great reſource in flat countries ſubject to inundations; becauſe thereby all the declivity of a great extent of the river, through its turns and windings, may be thrown into a ſmall ſpace by cutting a new channel in a ſtraight line; as may generally be done without obſtacle in ſuch countries as I am ſpeaking of, and hereby the velocity of the current will be very greatly augmented, and the back-waters carried off to a ſurprizing degree, as is evident from what is ſaid above in N° 29.

5thly,

5thly, Wherever there is a confluence of rivers or canals, the angle of their junction muſt be made as acute as poſſible, or elſe the worſt of conſequences will ariſe from the corroſion of their reſpective ſtreams; what they carry off from the ſides will be thrown into irregular banks in the bottom of the bed. This acute angle of junction may always be procured by taking the direction at ſome diſtance from the point of confluence.

6thly, Wherever the ſides or banks of a river are liable to a more particular corroſion, either from the confluence of ſtreams, or from irremediable windings and turns in the channel, they muſt be ſecured againſt it as much as poſſible by *weirs:* for this corroſion not only deſtroys the banks, and alters by degrees the courſe of the river, but alſo fills up the bed, and thereby produces all the bad effects we have ſpoken of above in N° 33. 34. &c.

7thly, But the principal and greateſt attention in digging the beds of rivers and canals muſt be had to the *quantity and form of their declivity*. This muſt be done uniformly throughout their whole extent, or ſo much of it as is neceſſary for the purpoſes in hand, according to the principles laid down above (in N° 29 and 30.) Conformable thereto, the depths of their beds, and of the floors of their ſluices, at the mouths whereby they diſ-

charge

charge their waters, being fixed according to what we have ſaid in N° 37. the depth of the reſt of the beds, and the quantity of declivity therein, muſt be regulated in conſequence thereof, ſo as to increaſe regularly the quantity of declivity in equal ſpaces the farther we recede from their mouths, and proceed towards their ſources or to the part where the regular current is to take place.

If the depth and volume of water in a river or canal is conſiderable, it will ſuffice, in the part next the mouth, to allow one foot perpendicular of declivity through ſix, eight, or even, according to DESCHALES [d], ten thouſand feet in horizontal extent; at moſt it muſt not be above one in ſix or ſeven thouſand. From hence the quantity of declivity in equal ſpaces muſt ſlowly and gradually increaſe as far as the current is to be made fit for navigation; but in ſuch a manner, as that at this upper end there may not be above one foot of perpendicular declivity in four thouſand feet of horizontal extent. If it be made greater than that in a regular bed containing a conſiderable volume of water, the current will be ſo ſtrong as to be found very unfit for the purpoſes of navigation, as will appear hereafter, when I come to inveſtigate the quantity of declivity in ſeveral rivers, the degree of ſwiftneſs of whoſe currents is well known.

(d) De Fontibus et Fluviis, prop. 49.

39. I dare boldly affirm, from the certain principles of hydrodynamics laid down in this eſſay, that if the above mentioned things (N° 37. 38.) were carried into execution in a proper manner; the velocity of currents and the acceleration of motion of the waters in rivers, and in canals when their ſluices are open, might be increaſed to any degree that can be required for opening their beds, and for preventing inundations during great rains or ſudden floods: by carrying off more ſwiftly the great acceſſion of water which then takes place. It would not be difficult, by theſe means, to increaſe the velocity of the current to double and triple what it is in rivers and canals, whoſe beds for a long ſpace of time have been left to themſelves. There is not, perhaps, a country on earth but what might be freed from inundations by theſe means. But it may be objected, that if all I have adviſed was put in execution, even in the flatteſt countries, the currents of rivers (for canals ſhut up with ſluices are here out of the queſtion) would become incommodious, if not unfit, for navigation, eſpecially againſt their ſtreams. This objection would be of weight if it was not evident that the various means which I have pointed out may be executed in whole or in part, to a certain degree, and no farther than neceſſary for the purpoſes required. But, as it is certain that a ſtrong and

regular

regular current in a river is the beſt of all means for keeping it open and deep, and for preventing the formation of banks in the bed by the ſubſidency of mud, &c. which it does not allow time to precipitate; I leave it to be conſidered, whether it is better to have a free and open navigation ſomething incommoded by the ſtrength of the current, or to have ſoon no navigation at all, without repeatedly digging the bed anew.

40. I ſhall not here enter into the mechanical part of the methods of digging and cleaning canals, rivers, and ſea ports, or into any deſcription of the machines and inſtruments neceſſary for that purpoſe. The ſubject would lead me much too far: beſides all theſe things may be found much at length in moſt of the authors who have wrote upon hydraulic-architecture, ſuch as BARATTERI, CORNELIO MEYERI, GUGLIELMINI, and *a notorious anonymous French plagiary*, who has taken from MEYERI, without ever naming him, almoſt all that is contained in his book, publiſhed at Paris in 1693, and at Amſterdam in 1696, in octavo, under the title of *Traité des Moyens de rendre les Riviéres navigables*. But the author who has treated this ſubject with the greateſt care, and moſt at length, is the celebrated BELIDOR, in his *Architecture Hydraulique*, 4 vol. in quarto. To theſe may be added a late memoir of M. FORFAIT of

Rouen, vice-architect of the French navy, which gained the prize of the Royal Academy of Sciences and Belles Lettres of Mantua, for having given the beſt ſolution of a problem propoſed by that Society in 1776, in the following terms: "*To indicate the beſt and cheapeſt method* "*of freeing navigable canals from banks of ſand and* "*earth formed in their beds which render them too ſhal-*"*low.*" This piece, printed at Mantua, by Pazzoni, in 1778, contains ſixty-three pages in quarto, and is divided into two parts; the firſt contains the means of preventing the formation of banks in navigable canals; and the ſecond offers divers methods for remedying them when they are already formed. For this purpoſe the author propoſes ſix different machines of his own invention: the firſt may be employed in rivers near the ſea, and ſubject to the ebb and flow of the tides; the ſecond may be uſed in thoſe where the waters are always nearly of the ſame height and velocity; the third and fourth are to be uſed in thoſe places where the violence of the currents corrode the beds; and the two laſt ſerve to break up the banks of ſand or earth formed in the bottom, and to carry off all heterogeneous bodies ſunk in the river, which cauſe an obſtacle to the current. It would be difficult to give a juſt idea of theſe machines without the help of the ſix plates which accompany the piece;

piece; but as this production of a foreigner has been crowned in Italy, the country of all others in which, from all antiquity, the ſcience of rivers and canals has been moſt cultivated, we cannot well doubt of its merit, or that it is worthy of a tranſlation into our own language.

V. *Other conſiderations on the nature of rivers and inundations.*

41. Rivers flowing along plains, as well as through vallies, have naturally their beds in the loweſt part of the ground comprized between the oppoſite hills or mountains: nevertheleſs, the ſurface of the water of a river in the midſt of a plain is often higher than the ſurface of the grounds adjacent to the banks of the river. This proceeds from the continual ſubſiding of the mud, &c. brought down by the ſtream during floods; the waters in that caſe uſually overflowing the banks ſpread themſelves over the plain, where they loſe a great part of the ſwiftneſs of their current, which contributes greatly to the ſubſiding of the mud they contain; ſo that the farther they flow upon the plain, the clearer they grow, and the leſs remains to ſubſide. From hence the greateſt precipitation of mud muſt be in the parts of the plain neareſt the ſides of the river, which in length of time will raiſe

theſe grounds above the reſt of the plain. Again, the waters in the bed itſelf depoſing inceſſantly a part of the mud, &c. brought down by the ſtream, muſt continually, though inſenſibly (for a long ſpace of time) raiſe the channel and banks of the river above the reſt of the plain. Theſe cauſes may at laſt contribute to the forming of an intire new bed for the river; for as all rivers carry down in their ſtreams more or leſs mud and other heterogeneous matters, which do not ſubſide regularly in all parts alike, but muſt precipitate faſteſt where the current is floweſt; there muſt accumulate by little and little in theſe parts ſuch banks of ſand and mud, as will in time hinder the current of the waters, make them reflow, and at laſt totally change their direction.

Canals are ſtill more ſubject than rivers to have their beds raiſed and their currents ſtopped by the ſubſiding of mud and heterogeneous matter in different places, and eſpecially juſt above their ſluices; becauſe of the ſudden ſtagnation of the water which firſt begins there as often as the ſluices are ſhut: and as there is a neceſſity for keeping them for the moſt part ſhut, the ſtagnating waters in their beds muſt precipitate their mud, &c. in a much greater proportion than can be done in the currents of rivers, which are in a continual motion towards the ſea.

42. I call *center of the current*, or, more properly, *line of greateſt current*, that *line which paſſes through all the ſections of a river, in the point where the velocity of the current is the greateſt of all.* We have ſeen above (N° 25.) that if the current of a river is regular, and in a right line, its center or line of greateſt velocity will be preciſely in the center of the ſections (N° 6.): but, on the contrary, if the bed is irregular and full of turns and windings, the center or line of greateſt current will likewiſe be irregular, and often change its diſtance and direction with regard to the centers of the ſections through which the waters flow, approaching ſucceſſively, and more or leſs, to all parts of the bed, but always in proportion and conformably to the irregularities in the bed itſelf.

This deviation of *the line of greateſt current* from the centers of the ſections through which it paſſes, is a cauſe of many and great changes in the beds of rivers, ſuch as the following:

1ſt, In a ſtraight and regular bed, the greateſt corroſion of the current will be in the middle of the bottom of the bed; becauſe it is that part which is neareſt to the line of greateſt current, and at the ſame time which is moſt acted upon by the perpendicular compreſſion of the water. In this caſe, whatever matters are carried off from the bottom will be thrown, by the force of the current,

current, equally towards the two ſides, where the velocity of the ſtream is the leaſt in the whole ſection.

2dly, If the bed is irregular and winding, the line of greateſt current will be thrown towards one ſide of the river, where its greateſt force will be exerted in proportion to the local cauſes which turn it aſide: in ſhort turns of a river there will be a gyration, or turning round of the ſtream, by reaſon of its beating againſt the outer ſide of the angle; this part will be corroded away, and the bottom near it excavated to a great depth. The matters, ſo carried off, will be thrown againſt the oppoſite bank of the river where the current is the leaſt, and produce a new ground, called an *alluvion*.

3dly, Inequalities at the bottom of a river retain and diminiſh the velocity of the water, and ſometimes may be ſo great as to make them reflow: all theſe effects contribute to the ſubſiding of ſand, earth, and other matters thereon, which ceaſe not to augment the volume of the obſtacles themſelves, and produce ſhallows and banks in the channel. Theſe in time, and by a continuance of the cauſes, may become iſlands, and ſo produce great and permanent changes and irregularities in the beds of rivers.

4thly, *The percuſſions of the center of the current* againſt the ſide of the bed are ſo much the greater as they are made

made under a greater angle of incidence; from whence it follows, that the force of percuſſion, and the quantity of corroſion and of detriment done to the banks and weirs of rivers, and to the walls of buildings made therein, and which are expoſed to that percuſſion, *are always in a direct compound proportion of the angle of incidence, of the greatneſs and depth of the ſection together, and of the quantity of velocity of the current.*

5thly, It may happen in time, that the excavation of the bottom, and the corroſion of the ſides, will have ſo changed the form of the bed as to bring the force of percuſſion into equilibrium with the velocity and direction of the current; in that caſe, all farther corroſion and excavation of the bed ceaſes (N° 31.)

6thly, This gives the reaſon why when one river falls into another almoſt in a perpendicular direction, and makes with it too great an angle of incidence, this direction is changed in time, by corroſions and alluvions, into an angle much more acute, till the whole comes into equilibrium.

7thly, So great and ſuch continued irregularities, from local cauſes, may happen in the motion of a river, as will intirely change its ancient bed, corrode through the banks, where they are expoſed to the greateſt violence of

percuſſion

percuſſion of the ſtream, and open new beds in grounds lower than what the old one is become.

8thly, Hereupon the ſtate of the old bed will entirely depend on the quantity of water, and on the velocity and direction of the current in the new one; for immediately after this diviſion of the waters into two beds is made, the velocity of the current in the old one will be diminiſhed in proportion to its leſs depth. In conſequence thereof, the waters therein will precipitate more of their mud, &c. in equal ſpaces than they did before; which will more and more raiſe up the bottom, ſometimes even till it becomes equal with the ſurface of the ſtream. In this caſe, all the water of the river will paſs into the new bed, and the old one will remain intirely dry. It is well known, that this has happened to the Rhine near Leyden, and to many other rivers.

9thly, Hence the cauſe of the formation of the new branches and mouth, whereby many great rivers diſcharge their waters into the ſea.

43. But in proportion as *a river, that has none of theſe obſtacles in its bed*, approaches towards its mouth, we ſee the velocity of its current augment, at the ſame time that the declivity of the bed diminiſhes, the cauſes of which have been explained above (N° 29.). It is for this reaſon, that inundations are more frequent and conſi-

derable, and do more damage in the interior parts of a country, than towards the mouths of moſt rivers.

In the Po, for example, the height of the banks made to keep in the waters diminiſhes as the river approaches to the ſea. At Ferrara they are twenty feet high; whereas nearer the ſea they do not exceed ten or twelve feet, although the channel of the river is not larger in one place than in the other.

44. The mouths of rivers, by which they diſcharge their waters into the ſea, are liable to great variations, which produce many changes in them.

1ſt, The velocity and direction of the current at theſe mouths are in a continual variation, cauſed by the tides, which alternately retard and accelerate the ſtream.

2dly, During the flowing of the tide, the current of the river is firſt ſtopped, then turned into a direction intirely contrary throughout a conſiderable extent; if we may believe M. DE BUFFON, there are rivers in which the effect of the tides is ſenſible at 150 or 200 leagues from the ſea.

3dly, This ſtate of things is a cauſe of a great quantity of ſand, mud, &c. being precipitated and accumulated in the channel near the mouth. This continually raiſes and widens the bed, and at laſt changes it intirely into a new place, or at leaſt opens new mouths to diſ-

charge the waters at. The Rhine, the Danube, the Wolga, the Indus, the Ganges, the Nile, the Miſſiſippi, and many other rivers, are inſtances of this.

4thly, All theſe effects are leſs ſenſible at the mouths of little rivers, as their currents oppoſe no ſenſible obſtacle to the flowing of the tides; ſo that the ebb carries off again what the flow had brought in.

45. Whenever the courſe of a river throughout a conderable extent of country approaches towards a right line, its current will have a very great rapidity; and the velocity wherewith it runs diminiſhing the effect of its natural gravitation, the middle of the current will riſe up, and the ſurface of the river will form a convex curve of ſufficient elevation to be perceived by the eye; the higheſt point of this curve is always directly above *the line of greateſt current* in the ſtream.

On the contrary, when rivers approach near enough to their mouths for a ſenſible effect to be produced in them by the flowing of the tides; and alſo when in other parts of their courſe they meet with obſtacles at the ſides of their channel: in both theſe caſes the ſurface of the water at the ſides of the current is higher than in the middle, even though the ſtream be rapid. In this ſituation of things, the ſurface of the river forms a concave curve, the loweſt point of which, or that of inflexion, is

directly over the line of greateſt current. The reaſon thereof is, that there are in this caſe two different and oppoſite currents in the river; that whereby the waters flow towards the ſea, and preſerve their motion therein even to a conſiderable diſtance; and that of the waters which remount, either by the flowing of the tide, or by their meeting with local obſtacles, which form a *counter current*, ſo much the more ſenſible as the flowing of the tide is ſtronger, or as the percuſſion of the water is made againſt greater obſtacles, and in a direction nearer to a perpendicular to them. From both theſe cauſes, the greater of which by far is that of the tides, the water near the ſides of the channel, where the velocity of the deſcending ſtream is naturally the leaſt (N° 25), takes a contrary direction, and runs back in the river, while that in the middle continues to flow on towards the ſea. This counter current is what the French call *a remous*.

An iſland in the middle of a river produces the ſame effect as obſtacles at the ſides, regard being had to the difference of ſituation of each.

Eddies and *whirlpools* in rivers, in the center of which there appears a conical or ſpiral cavity, and about which the water turns with great rapidity and ſucks in whatever approaches it, proceed in general from the mutual percuſſion of theſe two counter currents; and the vacuity in

the middle is produced by the action of the centrifugal force, whereby the water endeavours to recede, in a direct ratio of its velocity, from the center about which it moves.

46. If rivers perſevered always nearly in the ſame ſtate (N° 5.) the beſt means of diminiſhing the velocity of the current, when it is found too great for the purpoſes of navigation, would be by widening the canal: but as all rivers are ſubject to frequent increaſe and diminution, and conſequently to very different degrees of velocity and force in the current, this method is liable to produce very detrimental effects; for, when the waters are low, if the channel is very large in proportion, the ſtream will excavate a particular bed, which, according to the irregularities of the bottom, will form various turnings and windings with regard to the principal bed; and when the waters come to increaſe, they will follow, to a certain degree, the directions which the bottom waters take in this particular bed, and thereby will ſtrike againſt the ſides of the channel, ſo as to deſtroy the banks and cauſe great damages.

It would be poſſible to prevent in part the bad effects proceeding from the current ſtriking againſt the banks, by opening, at thoſe places where it ſtrikes, little gulfs into the land, dug in ſuch a form and direction as that the

ſtriking

ſtriking current ſhould enter and circulate therein, ſo as to deſtroy, or at leaſt greatly diminiſh, its velocity. This effect would be felt for a conſiderable way down the river.

This ſame method might probably be uſed with ſucceſs againſt the deſtruction of bridges, weirs, &c. by the violence of the ſtream during floods. Such gulfs being dug into the outer ſide of thoſe turnings in the river which are immediately above the place to be ſecured from the violence of the ſtream, would ſucceſſively diminiſh its velocity, its force and dangerous effects, a conſiderable way down the river. It is true, this method might contribute to produce an overflowing of the river upon the grounds adjacent to thoſe artificial gulfs, this being a natural conſequence of the decreaſe of the velocity of the current in thoſe places; and it would remain to be conſidered whether thoſe local inundations, or the danger of deſtruction of the bridges or edifices in the river, were the leſſer evil.

47. The nature of inundations, and the manner of their formation, merit a particular attention in this place.

While the volume of water in the *bed* of a river increaſes, the velocity of the current increaſes in proportion, as has been repeatedly ſhewn above (N° 13. 18. 20.

23. 27. 28. 29.). But from the moment that part of this water overflows the bed, the velocity thereof begins to diminiſh (N° 41.) and does ſo more and more, the farther it flows and ſpreads on the plain. So that the overflowing being once begun, it is a natural conſequence, that the inundation ſhould continue for ſeveral days; for though the volume of water brought down by the flood during that time ſhould decreaſe, yet, as the quantity of what runs off decreaſes likewiſe, from the great decreaſe of velocity in what overflows the plains, it will continue to produce the ſame effect as if the volume of water coming down had not diminiſhed, until the whole of the ſtream be every where contained again within the bed of the river. When that is become the caſe, the waters that have overflowed the plain will decreaſe thereon, by gradually and ſlowly running off, and alſo by evaporation, till they wholly diſappear. If this was not ſo, we ſhould ſee rivers overflow for an hour or two, and then return again within their beds, a thing contrary to general obſervation; for we conſtantly ſee inundations, once begun in flat countries, laſt for ſeveral days together, although in the mean while the rain ceaſes, and the quantity of water coming down diminiſhes. This muſt be the caſe, becauſe as the overflowing diminiſhes the velocity, and conſequently the quantity of water

carried

carried off, it has the ſame effect as if a greater quantity ſtill continued to come down.

It may not be uſeleſs to remark here, that what we have often ſaid in this eſſay becomes evident from theſe obſervations on nature, as well as from the principles laid down in it; to wit, that the moſt direct and efficacious method of preventing inundations is by deepening the bed and raiſing the banks of the river.

It may likewiſe be obſerved, with regard to inundations, that if the wind blows directly contrary to the current of the river, the overflowing will be greater than it would have been otherwiſe, becauſe this accident diminiſhes the velocity of the ſtream: but, on the contrary, if the winds blow in the ſame direction with the current of the river, the inundation will be leſs than otherwiſe, and ſooner at an end; becauſe this accidental cauſe augments the velocity of the ſtream.

VI. *On the confluence of rivers, and on the ſeparation of the ſame river into divers branches and mouths, with the effects thereof upon the velocity of currents, inundations, &c.*

48. All great and long rivers receive into their beds many others of different magnitude throughout the extent

tent of their courſe. This is evident to every one who only caſts his eyes over a map. The Rhine and the Po, in particular, receive each above one hundred others great and ſmall; the Danube above two hundred; the Wolga as many; the river of Amazons receives into its vaſt bed a prodigious number, ſome of which are five or ſix hundred leagues in length, and are of ſuch a depth and breadth as would make them elſewhere paſs for capital rivers. M. DE BUFFON (e) gives a liſt of the more conſiderable of thoſe which fall into other great rivers throughout the known world. Many curious particulars may be ſeen in VARENIUS's General Geography, part I. chap. XVI. concerning rivers; but of a nature which does not enter into my plan. The works themſelves are in every body's hands, and may be conſulted by thoſe who pleaſe.

This confluence of rivers is ſo neceſſary for propagating the motion of the water throughout a long courſe, and for renewing and accelerating from time to time its velocity, which otherwiſe would be too greatly diminiſhed by the reſiſtance of ſo many obſtacles as they meet with in their way, that, as we have ſaid above (N° 28.) after Signor GUGLIELMINI, it can only be attributed

(e) Hiſt. Nat. tom. II. p. 75, 76.

to

to the infinite wiſdom of the Author of Nature, in the original diſpoſition of things.

49. We have ſeen above (N° 18. 27. 28.) that the increaſe of a river or canal by the new waters which it receives, *is univerſally in an inverſe ratio of the new velocity which is acquired therefrom.* If this velocity is greater, the increaſe of the ſection of the new ſtream will be leſs in proportion, and *vice verſâ.* It follows from hence, that it is poſſible for one river or open canal to fall into another river or open canal of equal magnitude with itſelf, and yet the ſection of the current in the common bed after their confluence ſhall be no greater than it was in each of them before their junction. It is certain that this will be the caſe as often as *the confluence of the two augments the velocity of the common current in the ſame proportion with the increaſe of the volume of waters*; both the greater rapidity of the current, and the greater volume of water in the bed after the junction, ſerving to deepen it in proportion to its breadth, will contribute towards the above effect. Another cauſe will likewiſe add thereto; to wit, that inſtead of the reſiſtance from the attraction, friction, and other obſtacles, in two beds, which give two bottoms and four ſides, there are, after the confluence, only thoſe of one bed, of one bottom and two ſides. Moreover, the center of the ſection in the common bed

is farther from the bottom and ſides thereof, than it is in the ſeparate beds. All theſe cauſes, in proportion to their reſpective quantities, contribute to accelerate the velocity of the common ſtream.

50. It is not leſs certain, that in rivers which bring down a great abundance of water, the more the velocity and diſcharge thereof at their mouths are retarded and diminiſhed by the tides, the winds, the rolling in of the ſea, &c. the more will the back-waters increaſe in height, and endanger overflowing the inner parts of the country. This is evident, becauſe the decreaſe of velocity in the current, and the increaſe of height of all the back-waters that are affected thereby, are in a reciprocal inverſe ratio one of another (ſee above N° 32.).

Nature itſelf teaches us a method of preventing, or at leaſt of diminiſhing, this effect. We ſee all great rivers overcharged with a vaſt volume of water divide, when they come near the ſea, into different branches and mouths, whereby the ſuper-abundance of their waters is diſcharged. This is the caſe with the Scheld, the Rhine, the Rhone, the Po, the Danube, the Wolga, the Euphrates, the Indus, the Ganges, the Nile, the Niger, the Oroonoko, the River of Amazons, and with almoſt all other great rivers.

This ſeparation and diſperſion of the too great quantity of water into ſeveral channels is one cauſe of their ſeldom overflowing the country near their mouths (f), becauſe it gives a greater depreſſion and declivity to the ſurface of the current, and thereby facilitates the running down of the waters from the interior parts of the country, foraſmuch as their beds are every where regular and free from obſtacles to their current.

51. Notwithſtanding the apparent oppoſition to what has been ſaid in ſeveral other parts of this treatiſe, I repeat again, that this diviſion and diſperſion of the waters into ſeveral branches and channels *when there is ſuch an abundance of it as is ſufficient to keep up the velocity both in the old and new channels*, augments the declivity, and thereby facilitates the running off of all the back-waters from the inner parts of the country, as far as the bed is regular and free from obſtacles, according to what is laid down above (N° 38.).

But whenever this ſuper-abundance of waters, ſufficient for keeping up the velocity in each channel nearly to what it was before the ſeparation or divarication, ſhall be found wanting, it is certain, that this diviſion and diſperſion of the waters into ſeveral channels will only ſerve to diminiſh the velocity of the current in each, whereby

(f) Another cauſe thereof is pointed out above, N° 43.

as much or more diſcharge of the water, and conſequently of declivity for the running off of the back-waters, may be loſt, as has been gained by the ſeparation into different beds.

This diſadvantage may be eaſily remedied in thoſe new channels and mouths of rivers which are dug by hands, and have ſluices placed in them at the point of ſeparation from the original bed; for theſe ſluices of communication need be opened only when there is a ſuper-abundance of water in the river, ſufficient to keep up the velocity in each of the channels; at other times they may be kept ſhut, and the waters retained in their original bed.

52. It was for this purpoſe, of preventing the damages proceeding from immoderate inundations, that the ancient Egyptians dug vaſt lakes, and made ſo many canals and ſluices of communication between the Nile and thoſe lakes, and from thence to the ſea; that they might thereby be able to diſcharge the waters into thoſe reſervoirs if they came down in too great abundance, or let them off again from thence upon the land, if the quantity of the natural inundation at any time was leſs than what was neceſſary for the good of the country. By theſe means ancient Egypt was always maſter of its waters.

It is well known that it rains ſeldom in that country, and that the Nile by its regular inundations waters the land, by bringing down upon it the rains and melted ſnow from the high mountains of Abyſſinia. HERODOTUS (g) and DIODORUS SICULUS (h) have left us deſcriptions of the immenſe labours of the inhabitants to govern and multiply ſo beneficent a river, the particulars whereof are too well known to be repeated in this place. By theſe means Egypt became the granary of the world for above two thouſand years, and reimburſed, with immenſe advantage, the firſt expences.

RICCIOLI (i) aſſures us, that the ancient Perſians did the ſame thing with regard to the *Euphrates*, and for the ſame end. He adds, moreover, *Sic ubi Cyrus Gangem in Alveos 460 diſperſit, minora damna ex Gangis alluvionibus campi perpeſſi ſunt*; but I am totally at a loſs to find upon what authority he grounds this laſt aſſertion, for I never read that any CYRUS penetrated as far into India as the mouth of the *Ganges*, much leſs reigned ſo long over that country as to perform the vaſt work which RICCIOLI ſpeaks of.

(g) In lib. II.
(h) Biblioth. l. II. c. 1.
(i) Geogr. et Hydrogr. l. VI. cap. xxix. p. 248, 249.

PLINY

PLINY [k] ſays, with regard to the different mouths of the Po, *Omnia ea flumina foſſaſque primi à Sagi fecere Thuſci, egeſto amnis impetu per tranſverſum in Atrianorum Paludes, quæ Septem Maria appellantur.* Theſe ſeven lakes diſcharged their waters into the ſea by ſeven mouths, which PLINY names in the ſame place. All this was apparently done that the river might do leſs damage to the adjacent countries by its frequent inundations. PLINY adds, *His ſe Padus miſcet, ac per hæc effunditur, pleriſque, ut in Ægypto Nilus, quod vocant Delta.*

To theſe examples, drawn from ancient hiſtory, might be added many modern ones, if the things in queſtion had need of further proofs. Thus, both nature and the experience of a long ſeries of ages teach us, that the ſeparation of a river into ſeveral beds, by new branches and mouths, is a means of diminiſhing inundations in the inner part of the country; but that this takes place only when there is a ſufficient abundance of water in the river to fill the new beds and channels ſo far as to prevent the velocity of the currents therein from being notably diminiſhed from what they were before the diviſion.

[k] Hiſt. Nat. l. III. cap. XVI.

SEC-

SECTION II.

Laws of the meeting of oppoſite currents, with the application of them to ſluices.

53. When two equal currents of homogeneous fluids meet in oppoſite directions, there is firſt a ſwelling and riſing up of them at the point of rencounter; then follows a revulſion and counter current of each equally back again, ſo as to bring the whole to an equilibrium.

54. If the two oppoſite currents are unequal, either in force or in quantity, or in both, there will ſtill be a ſwelling and revulſion of each back again, but it will be diminiſhed in the greater current, and augmented in the leſſer, by the quantity by the which the one ſurpaſſes the other; and the point of rencounter of the two currents will have a ſlow and progreſſive motion in the direction of the ſtronger, the degree of velocity thereof being always in a direct ratio of the force and quantity of the one above the other,

55. If the fluids in oppoſite currents be not homogeneous, as is the caſe between ſea and river water, that which has the leaſt ſpecific gravity will ſwim upon the other, and continue to follow its firſt direction, until

ſuch

ſuch time as the heavier fluid ſhall have communicated its motion to all the parts of the lighter. But the lighter fluid will not loſe its former motion and direction at once, but *in a decreaſing ſeries, the law whereof will vary according to the greater or leſs difference of ſpecific gravity in the two fluids*, until the whole of the lighter has acquired the velocity and direction of the heavier which buoys it up.

The *time* and *ſpace* required for a greater current of ſalt water to communicate its motion and direction to an oppoſite one of freſh water will be but very little, ſince they differ in ſpecific gravity only $\frac{3}{73}$ parts that the ſalt is heavier than the freſh. It would require much greater between water and oil, and ſtill much more between quickſilver and oil, and ſo on. The elements for determining them in every caſe might be found by a proper number of experiments.

56. Let the two currents be equal or unequal in force and velocity *but nearly of the ſame ſpecific gravity*, if we ſhould ſuppoſe at the ſame time that their ſurfaces are not upon a level, but that the one is higher than the other (as is conſtantly the caſe in all ſluices that open to the ſea, except at the moment when the ſurface of the tide is upon a level with the ſurface of the water in the canal behind the ſluice); this circumſtance entirely changes both the caſe and the effects. It is certain, on this

this ſuppoſition, that the overplus of velocity and elevation in the higher current, though it ſhould be the leſſer, will make the waters in the lower and greater current reflow upon themſelves until they come to a level and equilibrium with thoſe in the upper current; ſince theſe, by the laws of univerſal gravitation, cannot flow back from a lower to a higher level, but muſt deſcend according to the declivity of the ſurfaces. If the currents are of *very different ſpecific gravities*, they will come to an equilibrium according to the law laid down above (N° 55); but their greater or leſſer quantity and velocity will produce little or no effect in this caſe.

57. Now as the running of two currents in oppoſite directions, after their rencounter, and beyond the limits laid down above (N° 55.), is incompatible with, and contradictory to, the laws of nature, and conſequently impoſſible; we may draw this uſeful concluſion, which becomes important during inundations, and eſpecially during the annual overflowing of the low grounds in flat countries; to wit, that if the ſluices next the ſea againſt which the tide flows be ſhut only a quarter of an hour before the *flood* has riſen to the level of the water in the canal, not a drop of ſalt water can enter the ſaid canal, nor even into the ſluice itſelf; becauſe both the progreſſive motion of *the point of rencounter of*

the two currents, and the *over-ſwimming* of the freſh water upon the ſalt, will be always without the ſluice and towards the ſea, ſo long as the ſurface of the tide is below the level of the water in the canal. Many ſluice maſters, for want of knowing or conſidering this, are accuſtomed to ſhut their gates next the ſea a little after *half flood*, under the pretence of preventing by this means the ſalt water from getting into the canal, and communicating thereby with the waters that overflow the low grounds in many places during winter, which would be of great detriment to the ſoil. Through this falſe perſuaſion, they loſe no inconſiderable part of that time every day, which they might ſafely employ in drawing off the waters which overflow and incommode low and flat countries almoſt every winter and rainy ſeaſon, as is the caſe in the Dutch and Auſtrian Netherlands.

SECTION IV.

Experiments to determine the different velocities, in different depths of water, of the ſame floating body moved uniformly by an equal force.

58. It is well known already, that for facilitating or retarding the motion of boats, &c. in canals, the different

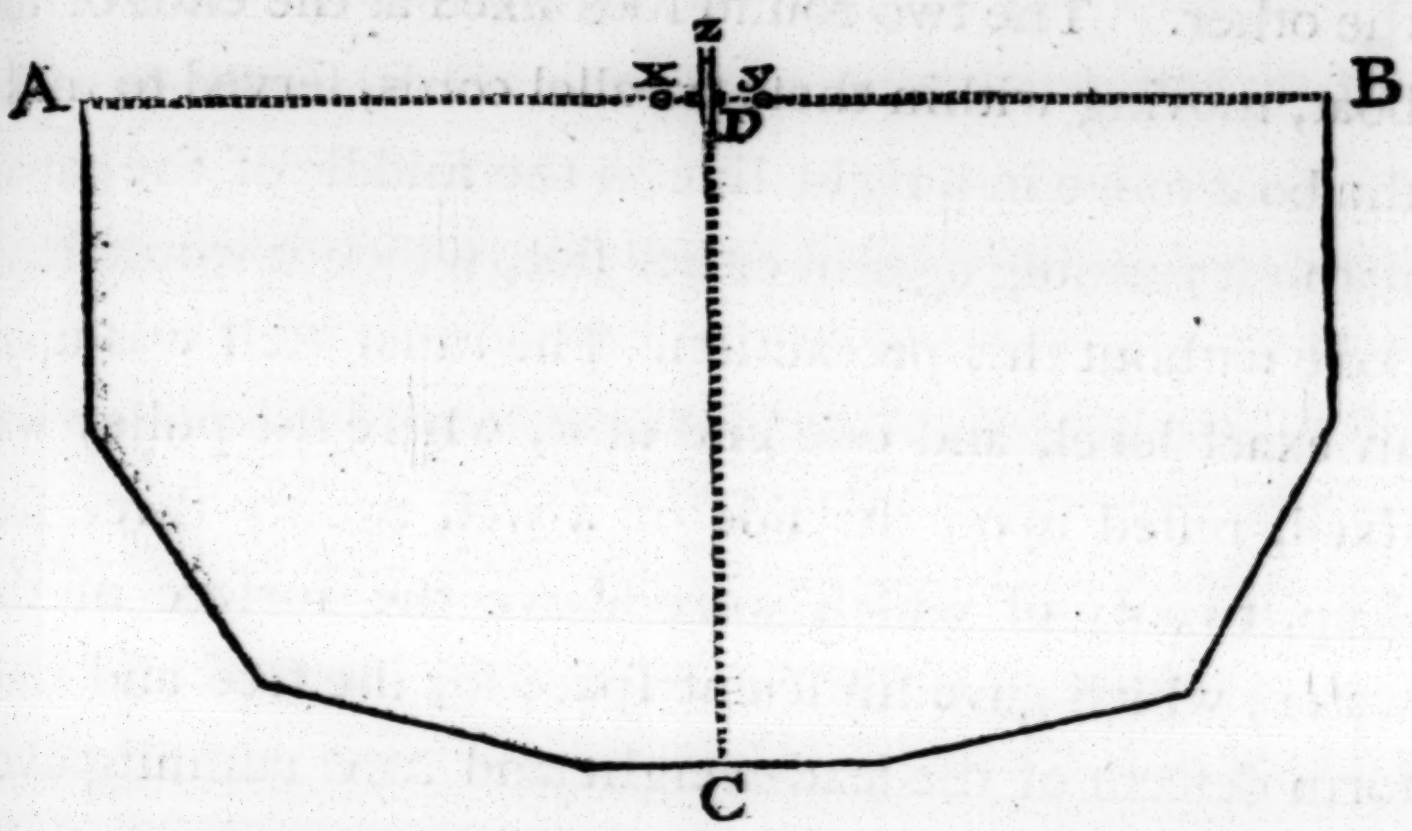
depths of the water, above that ſimply neceſſary to keep them afloat, is a thing not at all indifferent. Dr. FRANKLIN has already treated this ſubject, though perhaps not with ſufficient accuracy, in a letter to Sir JOHN PRINGLE, written in the year 1769. He proves, however, that it is univerſally known among people accuſtomed to work boats on canals, that there is a conſiderable difference in the ſwiftneſs of their motion according to the greater or leſs depth of the water therein; and that the water being low is of itſelf ſufficient to retard the motion of a boat, without the keel thereof rubbing againſt the bottom of the canal. The reaſon he aſſigns for it is evident; for a boat cannot advance its own length in a canal without diſplacing a quantity of water equal in maſs to the ſpace which the boat occupies under the ſurface of the fluid. The water ſo diſplaced muſt retrograde, and paſs under, and to the right and left, of the boat: ſo that the leſs depth and breadth of water there is in the channel, the more in proportion it muſt riſe up and weigh againſt the boat, and the more difficulty it muſt find in paſſing under and along ſide of it, and neceſſarily muſt retard ſo much the more the motion thereof. The reſult of Dr. FRANKLIN's experiments on this ſubject may be ſeen in the letter above mentioned.

59. Mr. NEEDHAM, Director of the Imperial Academy of Sciences at Bruxelles, being of opinion that Dr. FRANKLIN's experiments were made upon too ſmall a ſcale to draw any very exact inferences from them, deſired me, at the beginning of the year 1775, to make a new ſet of experiments upon a much larger ſcale and with all poſſible exactneſs; I did accordingly, and ſhall here give a ſhort deſcription of them.

I got made, by the ſhip carpenters of Nieuport upon the Coaſt of Flanders, an exact model of a bilander, anſwerable in all its proportions to thoſe uſed in the Low Countries. Its length was thirty-nine Engliſh inches, its breadth nine inches and a half, and its depth nine inches. Its form both within and without exactly repreſented that of a bilander. At each end of it was faſtened perpendicularly a round and poliſhed rod, ten inches and and a half in height above the ſides of the boat.

I got made likewiſe a wooden canal, twenty feet in length, thirty-ſeven inches in breadth, and ſixteen inches in depth; a ſection of which is repreſented in the following figure:

This



This form is that of the excavation of the canals in the Low Countries, and approaches to that of the natural beds of rivers inaſmuch as they are regular. Here AB=37 Engliſh inches, DC=16 inches; and the length of the whole canal, as we ſaid before, was twenty feet. z repreſents the ſection of a pulley fixed at one end of the canal, upon which paſſed a ſmall cord, one end of which was tied to the round rod at the fore part of the boat, and at the other end was a piece of lead which weighed eight ounces. This ſerved for an equable force to give an uniform motion to the boat throughout all the experiments. *x* and *y* are ſections of two other cords ſtretched parallel to each other at about one inch and a half diſtance, and reaching from one end of the canal to

the other. The two round rods fixed at the ends of the boat, moving within theſe parallel cords, ſerved to make the boat move in a right line in the middle of the canal, without running againſt either ſide, which it would have done without this precaution. The canal itſelf was upon an exact level, and one end of it, where the pulley was fixed, reſted upon the ſide of a well twenty-three feet deep, twenty of which were above the ſurface of the water; which gave ſufficient ſpace for the free and uniform deſcent of the lead-weight and cord running over the pulley, as they drew the boat from one end of the canal to the other.

Latts, exactly divided into inches, were nailed againſt each end of the canal within, to mark the different depths of the water in it according as it ſhould be augmented or diminiſhed. The outſides of the little boat, from its keel upwards, were likewiſe divided into inches. In the inſide of the boat was a quantity of ſand ſufficient to ſink it to ſix inches deep in the water. The common loaded bilanders in the Low Countries uſually draw ſix feet of water.

Thus the form of the wooden canal, together with its breadth and depth, and the form and dimenſions of the little boat, together with the depth of water it drew by means of its ballaſt of ſand, exactly correſponded with

thoſe

thoſe in the real canals and bilanders in the Low Countries, an inch in the one anſwering to a foot in the other.

Cloſe to the canal, and out of the way of all wind, was ſuſpended a pendulum of fine waxed thread, to prevent the variations of the atmoſphere from altering its length, which from the point of ſuſpenſion to the center of gravity in the lead was $39\frac{1}{5}$ Engliſh inches, ſo that its iſochronic vibrations were exactly ſeconds of time.

60. It was neceſſary, in order to render the experiments exact, that they ſhould be made at a time when the air was perfectly calm; for the leaſt breath of wind, during the motion of the boat, cauſed great variations and irregularities in them, which it was abſolutely neceſſary to prevent, in order to be able to deduce any exact reſults from them. On the contrary, in a perfect calm, the times of the paſſage of the boat, from one end of the canal to the other, were exceedingly regular, as may be ſeen from the table of experiments which I give below.

By means of the pendulum I was able to meaſure the times of paſſage of the boat along the canal, in all the different depths of water, to a third or even to one quarter of a ſecond. The boat being held faſt againſt the back end of the canal by the hand of an aſſiſtant, and then let go, it was eaſy for me to perceive the preciſe

inſtant

inſtant of the beginning of its motion, to let go the pendulum at the ſame moment, and to count its vibrations till the inſtant that the boat ſtruck with an accelerated force againſt the fore end of the canal. As to the weight of eight ounces ſuſpended at the end of the cord, and which ſerved as *a moving force* to draw the boat along the canal, it was juſt as much as ſufficed to counterbalande the cord, and to put the boat in motion; leſs weight than that would do neither: therefore I was obliged to uſe ſo much, notwithſtanding the conſiderably accelerated motion it gave to the boat.

This is the whole mechaniſm of the inſtruments I uſed for the experiments in queſtion, and ſuch were the precautions I judged it neceſſary to uſe for making them with ſcrupulous exactneſs.

In the following table, which conſiſts of twelve columns, the firſt of them contains the different depths of water at which the experiments were made; the ten following ones contain ten different experiments made at each depth of water in the canal; and the twelfth or laſt column is the reduction of the ten others to a mean proportional or mean reſult of the whole, which is in *ſeconds of time.*

Table of experiments made to aſcertain the times of paſſage of the boat along the canal, or its different degrees of velocity, in different depths of water.

Depths of Water.	1ſt Exp.	2d ditto	3d ditto	4th ditto	5th ditto	6th ditto	7th ditto	8th ditto	9th ditto	10th ditto	Mean Reſults.
Inches	"	"	"	"	"	"	"	"	"	"	"
15	14½	14¾	14⅗	14½	14½	14⅗	14⅗	15	14½	14⅗	14⅗
14	14¾	15	15	15½	15	15	14½	14½	15	15¼	15
13	15½	15½	15½	15¾	15½	15¾	15¾	15½	15¼	15¼	15½
12	16½	16	16	15½	16	15¾	16	16	16	16	16
11	16¾	17	17	16½	16¾	17	16¾	16½	16¾	16¾	16¾
10	17	18 wind	17	17½	17	17⅓	17½	17	17	17¾	17½
9	18½	18½	18	19	19	18	18	18¾	18	18½	18½
8	20	20	20½	20	20	20	20	19½	20	20	20
7	23½	23	22½	23	23	23	23½	23¼	23	23	23
6½	In the experiments made with this depth of water, the boat often touched the bottom.										30 by ſuppoſition

It may be obſerved with regard to the laſt column of the above table, which contains the mean reſults or mean quantities of time which the boat takes to paſs from one end of the canal to the other in different depths of water, that it is given for the ſake of deſtroying thoſe little differences which are inevitable in practice; and it

ſhews, as nearly as poſſible, what the true time of paſſage ought regularly to be when nothing happens to diſturb it.

It is alſo highly worthy of remark, that the *mean reſults* contained in this laſt column form a *ſeries of numbers regularly increaſing as the depths of water, wherein the reſpective experiments were made, regularly decreaſe*; ſo that *the different velocities of the floating body are in an inverſe ratio of the reſpective depths of the water in which it floats with an equal impulſive force, and that according to the law of the above ſeries.* This, perhaps, may furniſh elements to calculate, pretty near the truth, the different velocities of veſſels upon canals and rivers with different depths of water in all other caſes whatſoever. As to the concluſions to be drawn therefrom in practice, and in the common uſes of life, they are too obvious to need mentioning here.

SECTION V.

On the quantitity of declivity in rivers.

62. Abſtracting from all reſiſtance and friction, fluids, ſuch as water, deſcend upon planes let them be never ſo little inclined towards the center of the earth: and the

velocity of deſcent increaſes in a compound ratio of the increaſe of the maſs of water, and of the greater declivity of the plane which ſerves for its bed (N° 13.).

63. Water, though unaffected by any compreſſion or impulſion from above, cannot remain immoveable in any bed whatever except that which makes a curve perfectly concentrical with the *terreſtrial curve*; but in this, being every where equally affected by the force of gravitation, it will remain without motion any way.

64. It follows from hence, that the ſprings and ſources of all rivers muſt be at a greater diſtance from the center of the earth than one ſemi-diameter thereof, which is terminated at the ſurface of the ſea; without which the waters could not run to the river's mouth.

65. Therefore, the abſolute elevation of the ſurface of rivers is continually diminiſhed as they recede from their ſprings, becauſe of the neceſſary declivity of the beds of rivers towards the center of the earth; for without ſome degree of this declivity the waters could not run at all, as has been ſaid above (N° 62. 63.).

66. The declivity of the beds of rivers cannot be a right line making a rectilinear angle with that horizontal plane which, being continued, would interſect their reſpective ſources; but, if it is regular, it muſt be a curve which differs very little from that of the earth's ſurface,

and this, if the direction is in the parallels of latitude due Eaſt and Weſt, is *ſpherical*; but in all other directions it is a portion of an *oblate ellipſis*, on account of the earth's being a ſpheroid compreſſed by its axis. Now the horizontal plane which continued paſſes through the ſprings of rivers, is always a tangent to the curves of their beds, at the point of inflexion, inſomuch as theſe are regular.

67. The quantity of abſolute declivity from the ſpring in any determinate part of a river, is that perpendicular line drawn from the point of greateſt current in that place till it meets *the curve concentrical to the earth's ſurface* which paſſes through the river's ſource. The declivity of the bed below the ſpring is had by taking the ſame perpendicular from the bottom of the bed; as that of the river's ſurface is by taking the perpendicular from thence.

68. If a plane be extended horizontally every way from the point of tangency to the earth's ſurface, or from the point where it is perpendicular to any radius of the earth, water will run from every other part of the plane towards that ſaid point which is nearer to the center of the earth than any other point in the whole plane.

69. The depreſſion of *the curve of a river's bed*, below *the concentrical-terreſtrial-curve which interſects its ſource*,

ſource, being only 250 fathom perpendicular in a courſe of 500 leagues, it will be ſufficient to give a notable current in a regular bed throughout all that extent of river, as appears from what we have ſaid above (N° 38.). But the depreſſion of *the curve of the river's bed*, below *the horizontal plane which is a tangent to its ſource*, in this ſame extent of courſe, is not leſs than ninety leagues perpendicular, being always *the ſecant of the arc of the river's extent minus a radius of the earth in that point.*

70. It follows evidently from the above princples (N° 62—69), that the declivity and velocity of a river are leſs in proportion as the bed approaches nearer to being concentrical with the curve of the earth's ſurface.

71. I ſhall now apply the principles laid down to determine, as near as poſſible, the real quantity of declivity in different rivers, making uſe of what is already known from experiments and actual menſuration to determine the ſame in all others by the compariſon of the different degrees of velocity in their reſpective currents.

72. It is the general opinion of moſt of thoſe who have examined this ſubject[k], that rivers and canals which have leſs than one foot of declivity in 10,000 feet of courſe, will have very little current, unleſs it be by means of the great abundance of their upper waters

(k) Vide DESCHALES, de Font. et Fluv.

which give motion to thoſe before them by their weight and impulſion. Without this the reſiſtance proceeding from the bottom and ſides of the bed, and from other accidental obſtacles (N° 21.) would equal, if not ſurpaſs, the ordinary cauſes of acceleration (N° 20) ſo as to diminiſh continually the motion of the waters, and at laſt render them almoſt ſtagnant (N° 22.). But nature has prepared remedies againſt this, as we have ſeen above (N° 28. 29. 48.). What RICCIOLI (l) ſays of the *Po*, in that part of its courſe next its mouth, is perfectly conformable to this theory: "Sic Padus, qui a Pago *Oſtellata* "vocato, uſque ad *Adriaticum*, intervallo milliarium cir- "citer 70, non habet libramentum majus 13 aut 14 "pedum, ita ut ſingulis milliaribus ne 3 quidem unciæ "declivitatis obveniant; unde *Paduſæ*, potius inſtar ſtag- "nantibus aquis, incertiſſimus eſſet ad defluxum curſus: "impetu tamen impreſſo à 30 et amplius fluminibus "aut torrentibus ſe in illum exonerantibus, etiamque à "nativæ pondere aquæ ex ſuperioribus et altioribus "prope Alpes alveis decurrentis, velocitatem maximam "acquirit."

73. From many obſervations and trials which I made for this purpoſe in the years 1773 and 1774 upon the river Iprelee in Flanders, which comes down from the

(l) Geogr. et Hydrogr. l. VI. c. xix. p. 215. edit. Bonon. 1661.

city

city of Ipres and falls into the ſea at Nieuport, having *a very moderate current* when the ſluices upon it are open, I found its *mean declivity* to be nearly three fathoms four feet and eight inches in 20,000 fathoms of extent of its courſe, or very nearly one foot in a meaſured Engliſh mile. I ſay its *mean declivity*, becauſe from what has been ſaid above (N° 13. 27. 28. 29.) it is plain, that a greater or leſs quantity than ordinary of water in it will add to, or take from, ſomething thereof; but the declivity in each part of its bed is nearly uniform.

As the ſources of this river, and thoſe of the Iſere which joins it at Fort Knock, four leagues from Nieuport, are in the higher grounds of Flanders towards Houthem, Mount Kemele, Swaertſberg, Catſberg, and the other hills as far as Mount Caſſel; and as the reſt of their courſe is in a flat country with a very ſmall deſcent towards the ſea, the declivity thereof may be taken as *a mean* between that of the other rivers and canals of Flanders: the *artificial canals* will have leſs, not above a ſix or ſeven thouſandth part of their extent, or one twelfth of an inch in each eight fathoms: the rivers Lys and Eſcaut, before they fall into the flat country, ſomething more, after which they may have about the ſame, or even ſomething leſs between Ghent and Antwerp.

This

This quantity of three fathoms four feet and eight inches of declivity in 20,000 fathoms of extent, gives the proportion of the declivity to the extent as 1 to 5292, which is one line or twelfth part of an inch in $6\frac{1}{8}$ fathoms, and two feet ſeven inches in one French league of 2283 fathoms. Now the meaſured Engliſh mile containing 5280 feet, this proportion of $\frac{1}{5292}$ approaches ſo very near to one foot of declivity in every meaſured mile of extent, that I ſhall every where reduce what I call the *mean declivity* to that quantity, as a ſtandard wherewith to compare the reſt.

74. In canals, all whoſe ſluices and vents have been kept ſhut a ſufficient time to render the water ſtagnant throughout their whole length, there cannot be allowed above an inch or two of declivity for each mile in length, on account of the water that unavoidably runs off through the chinks of the doors of ſluices, drains, &c.

75. According to the obſervations of the Abbé CHAPPE D'AUTEROCHE [m], the floor of the Hall of the Royal Obſervatory at Paris is forty-five fathoms three feet and five inches French above the level of the ſea at the mouth of the Seine. According to the Abbé NOLLET, this ſame floor is forty-ſix fathom above the level of the Ocean, and only forty-five fathoms above the level of the Mediter-

(m) See Relation de ſon Voyage en Siberie, tom. II. p. 406, 407. 444.

ranean

ranean ſea. Again, according to the above Abbé CHAPPE, the ſaid floor of the Obſervatory is elevated twenty-four fathoms one foot and ten inches above the level of the river Seine at Paris; therefore the level of the Seine under the Pont Royal at Paris is twenty-one fathoms one foot and ſeven inches above the level of the Ocean; and ſuch alſo is the quantity which Meſſ. CASSINI have given, from their own obſervations and experiments, for the mean height of the Seine at Paris above the level of the ſea.

Now the courſe of the Seine from Paris to its mouth at Havre de Grace, by following all its turns and windings, is about 90,000 fathoms in length; therefore $\frac{90,000}{21.1.7} = 4232\frac{1}{2}$ fathoms of extent for one fathom of declivity in the river Seine, or one line in $4\frac{2}{3}$ fathoms, and conſequently the proportion of its declivity to its length is as one to $4232\frac{1}{2}$. It is to be obſerved, that the bed of the Seine is deep, and its current conſiderably ſtrong.

76. By ſimilar obſervations and actual levellings made upon the river Loire by M. M. PICARD and PITOT [n], the declivity thereof in proportion to its length is found to be as one to 3174, which is one line in $3\frac{2}{3}$ fathoms. Notwithſtanding this great declivity of the bed of the Loire it is obſerved, that the velocity of the water therein,

(n) See Memoires de l'Acad. Royale des Sciences de Paris, pour 1730.

compared to that in the Seine, is leſs than it ſhould be in proportion to their reſpective declivities, which is very juſtly attributed to the much greater depth in proportion to the breadth of the Seine, above what is found in the Loire. This laſt river is remarkably broad, and ſo ſhallow that in many places it is hardly navigable for boats. Now this, according to the principles laid down above (N° 27, &c.) muſt very much diminiſh the ſwiftneſs of the current, which it ſhould otherwiſe have from the great declivity of its bed. In confirmation of this it is moreover obſerved, that in great falls of rain, which equally increaſe the volume of water in both theſe rivers, the velocity in the Loire augments in a much greater proportion than it does in the Seine; and this obſervation is likewiſe conformable to the principles above laid down (N° 12. 28. &c.).

77. The river Doux, after paſſing by Beſançon, falls into the Saone above Chalon; the Saone joins the Rhône at Lyons. This river, from Beſançon to its mouth in the Mediterranean ſea, is one of the moſt rapid in the known world: the velocity of its current is at leaſt double to that of the Seine or Loire, and its courſe is almoſt in a ſtraight line. The difference of elevation of this river at Beſançon, above that of its mouth in the Mediterranean ſea, after a courſe of about eighty-ſix French leagues, has been

been found, by a long ſeries of barometrical obſervations, to be about ſeventy-five fathom [o], which gives the proportion of the declivity to the extent as one to 2620, or about one third of a line to each fathom. This is double the *mean declivity* of the rivers in Flanders; but the velocity of the current in the Rhone is at leaſt triple that in the others (N° 29.).

78. From the above *data*, got from obſervations and actual menſuration, and from many others of the ſame nature too long to mention here, we may deduce the following table of comparative proportions between the declivities and velocities in different kinds of rivers.

(*o*) See Cours de Phyſique de Para, tom. II. N° 740.

Rates or claſſes of rivers and flowing waters.	Comparative degrees of the mean velocities of currents	Seconds of time wherein currents run 20 fathom.	Fathom run by the current in 1 minute of time.	Ratios of declivity compared with horizontal length.	Fathoms of length for each $\frac{1}{12}$ inch of declivity.	Diſtinctive attributes of the various kinds of rivers and flowing waters.
1	0	" 0	0	$\frac{1}{11000}$	14	Channels wherein the reſiſtance from the bed, and other obſtacles, equal the quantity of current acquired from the declivity; ſo that the waters would ſtagnate therein, were it not for the compreſſion and impulſion of the upper and back-waters.
2	$\frac{2}{3}$	180	$6\frac{2}{3}$	$\frac{1}{7000}$	8	Artificial canals in the Dutch and Auſtrian Netherlands.
3	1	120	10	$\frac{1}{5200}$	6	Rivers in low and flat countries, full of turns and windings, and of a very ſlow current, ſubject to frequent and laſting inundations.
4	$1\frac{1}{2}$	80	15	$\frac{1}{4000}$	$4\frac{2}{3}$	Rivers in moſt countries that are a mean between flat and hilly, which have a good current, but are ſubject to overflow: Alſo, the upper parts of rivers in flat countries.
5	$2\frac{1}{6}$	55	$21\frac{2}{3}$	$\frac{1}{3200}$	$3\frac{2}{3}$	Rivers in hilly countries, with a ſtrong current, and ſeldom ſubject to inundations: Alſo, all rivers near their ſources have this declivity and velocity, and often much more.
6	3	40	30	$\frac{1}{2600}$	3	Rivers in mountainous countries, having a rapid current and ſtraight courſe, and very rarely overflowing.
7	5	24	50	$\frac{1}{2000}$	$2\frac{1}{3}$	Rivers in their deſcent from among mountains down into the plains below, in which places they run torrent-wiſe.
8	8	15	80	$\frac{1}{1700}$	2	Abſolute torrents among mountains.

I ſhould

I ſhould think it quite ſuperfluous to give any explanation of a table ſo clear and intelligible as the above; and ſhall only remark upon it that *the comparative degrees of the mean velocities of the reſpective currents* in the ſecond column are the reſult of obſervations and experiments, the method of making which has been given above (N° 26.): but as the velocity of rivers is very different in different ſeaſons of the year, which augment or diminiſh greatly the maſs of waters in their beds, *a mean* has been kept to, as much as poſſible, in the above table.

By taking the degree of velocity of the current in any river, a thing ſo eaſy to be done; and obſerving its other characteriſtics as laid down above under the title of *diſtinctive attributes*, it will be eaſy to judge very nearly of *the quantity of declivity in that part of the river.*

79. After carefully comparing what has been ſaid in the relations of travellers, and in the beſt treatiſes of geography, upon the principal rivers in the known world, I ſhould be inclined to claſs them in the following manner, particularly entreating at the ſame time that my opinion about it may be regarded as ſimple conjecture, which I leave to be rectified by thoſe better acquainted with the matter than it is poſſible for me to be.

Under the firſt rate or claſs in the above table I ſhould put that part of the channel of moſt great rivers which

is

is in extenſive plains next the ſea; with regard to *the declivity thereof alone*, but not at all with regard to the velocity of the current there, which is often very great from the compreſſion and impulſion of the upper waters, as has been repeatedly ſhewn above muſt be the caſe (N° 29. 38. 43. 72.).

Second rate or claſs. Moſt artificial canals in flat countries, made for the uſe of navigation; eſpecially thoſe in the Dutch and Auſtrian Netherlands.

Third rate or claſs. The river Trent; the Scheld and the Lys below Ghent; the Iſere and the Iprelee below Fort Knock in Flanders; many rivers in the territories of Bologna and Ferrara in Italy; the river Meander in Natolia; and innumerable others in flat countries.

Fourth claſs. The Thames; the Lys and the Scheld above Ghent in Flanders; the Senne, the Dyle, and the Demmer, in Brabant; the Seine and the Somme in France; the Nile and the Niger in Africa; the rivers of St. Lawrence below Lake Ontario, the Oroonoko, the river of Amazons, and the rivers of Paraguay, in America.

Fifth claſs. The Severn and Ouſe in England; the Loire and Garonne in France; the Tagus, the Guadiana, and the Guadalquivir, in Spain; the Po and the Tiber in Italy; the Meuſe, the Rhine, and the Elbe, in Germany; the Weiſſel, the Neiſter, the Bog, and the Nieper,

in

in Poland; the Don and the Dwina in Ruſſia; the Amur or Saghalien in Tartary; the Yellow and Blue Rivers in China; the rivers of Cambodia, Ava, and Ganges, in India; the Euphrates; the river Zaire in Congo; the Miſſiſippi.

Sixth claſs. The Rhone in France; the Ebro and Douro in Spain; the Danube; the Wolga; the Irtiſch and Oby, the Jeneſea and Lena, in Siberia; the river Indus; the Tigris; the Malmiſtra in Cilicia.

Seventh claſs. In this claſs can only be enumerated thoſe parts of rivers where they deſcend from among mountains into the plain country below; as alſo ſome rivers paſſing through the midſt of mountains.

Eighth claſs. To this claſs belong all torrents among mountains; ſuch, for example, as the Bourns in the Highlands of Scotland are deſcribed to be.

SECTION VI.

A general and eaſy method of taking levels through large extents of country where rivers paſs; and alſo of computing the heights of interior parts of continents above the ſurface of the ſea.

80. After all I have ſaid hitherto in this eſſay, and particularly in the foregoing ſection, what I am about to lay

lay down under this laſt head of it muſt appear very plain and eaſy. I am very far, however, from giving the methods I am going to propoſe *for taking the levels through whole countries and continents as far as rivers extend*, as ſtrictly exact; I know very well that it is next to impoſſible they ſhould be ſo, conſidering the continual variations in the declivities of rivers, and in the velocities of their currents in different parts, as alſo the impoſſibility of knowing the exact length of their courſe through all their turns and windings I only give them therefore as a general and eaſy method of computing the relative heights of countries without deviating much from the truth, which, perhaps, is all that may be neceſſary for the conſideration of the natural philoſopher. At all events, they may be of ſome uſe, for this end, in ſo many parts of the earth through which rivers paſs, and where no barometrical obſervations, or any others whatever, for taking heights above the ſea, have been, or perhaps ever will be made. They may alſo be found uſeful in taking the levels through a large extent of flat countries where regular canals and rivers paſs, and where the difference of elevation is too ſmall to be obſerved by the barometer, and where alſo the taking them through ſo great an extent by the common methods of levelling would be much too expenſive for the purpoſes required. Now in

this laſt caſe I have found, by experience, that by the method I here propoſe the difference of heights may very eaſily be found, and that very near to the truth.

For this end it may be proper to premiſe a few neceſſary conſiderations and precautions to be obſerved in making uſe of the method I here propoſe. They would eaſily occur to any one who conſiders the principles whereon it is grounded; but to ſave trouble I ſhall put them down in a few words.

81. The firſt is, that a particular attention muſt be had to the quantity of water actually in the river at the time of the operation, ſo that according as the greater or leſs quantity thereof may augment or diminiſh the velocity of the current, allowance may be made conformable thereto in determining the quantity of declivity from the degree of velocity.

2dly, Obſerving this precaution throughout the whole river, or all that part of it wherein we want to find the difference of elevations, we muſt next endeavour to determine, as near as poſſible, by the principles laid down in the laſt ſection, *all the variations of declivity from the variations of velocity within thoſe limits, and alſo the exact length and quantity of each.*

3dly, The ſame attention muſt be had in taking the difference of heights by canals, while their ſluices and

communications are kept conſtantly open, ſo as to effectuate a compleat natural current throughout the whole extent thereof; for in this caſe they are no other than rivers, and their waters follow the ſame laws of motion.

4thly, But in canals which are ſhut, and their waters kept up by ſluices ſo as to render them nearly ſtagnant, the practice of this method will be different from what it is in rivers and open canals: for in this caſe there cannot be allowed for the declivity of the ſurface of the water from ſluice to ſluice above one inch, or two at moſt, in each mile of length, according as there may be fewer or more accidental drainings of the water in it (N° 74.).

Again, as it may happen, in taking the levels of countries by the means of artificial canals, that the water in different parts may have different directions, attention muſt be had to *add* or *ſubtract* reſpectively the total declivity of each.

Moreover, it almoſt always happens, in canals where the ſluices are ſhut, that the water on the two ſides of each ſluice is of a very different height, the back waters being kept up, while the lower are run off to a certain point; but in ſluices next the ſea, the tide againſt the outer gates is ſometimes lower and ſometimes higher than the water in the canal above. In all theſe caſes, the difference of height muſt be exactly meaſured, and the

quantity

quantity respectively added or subtracted in the account of the levelling.

5thly, After this it is neceſſary to determine, as nearly as poſſible, the length of the canals and rivers through all their turns and windings, and throughout the whole extent of country in which we want the difference of elevations. This may be done by an actual menſuration, or by the general opinion of the inhabitants of each part of the country, which, being founded upon the long and continually repeated experience of an infinity of people, will be found to differ very little from the truth, *attention being had to the quantity of their nominal meaſures*; even the errors in *more* or *leſs* will nearly compenſate each other; or, finally, in great extents it may ſuffice to compute them from good geographical maps.

6thly, This being done by one or the other of theſe methods, it will be eaſy, from the quantity of declivity before determined for each part in particular, to find the whole quantity of declivity throughout the whole extent of country meaſured, or from any one part thereof to any other along the rivers or canals in queſtion, which are ſuppoſed to be continued without interruption from one place to the other. If to this be added the relative height of the country in each place compared with the level of the water in the part of the river or canal next to each,

we ſhall have very nearly the difference of elevation of thoſe two parts of the country. And thus the levels may be taken from the ſea through any extent of country, nay even through whole continents, as far as rivers or canals extend without interruption. Cataracts themſelves, ſuch as thoſe in the Nile and in the river of St. Lawrence, need not hinder the operation, ſince we have only to take the reſpective heights from which they fall into the account as we do in common ſluices, and allow for the increaſe of velocity produced by them in the current of the river above and below the places where they exiſt.

82. Although I do not pretend to equal this method (of finding the difference of heights in countries) *for exactneſs* to the levels taken by actual menſuration, or to thoſe found by a long ſeries of nice barometrical obſervations; yet it muſt be allowed, that it is free from many inconveniencies, and accompanied with many conveniences, which the others are not. It may be eaſily carried through great extents of country, where the other methods cannot be put in practice, on account of the expence or time required; and this may be done with very little trouble, and perhaps with ſufficient exactneſs to anſwer all the purpoſes of the natural philoſopher in his conſiderations on the globe we inhabit. Although the method of taking heights by barometrical

obſervations

obſervations is highly uſeful, and among mountains (where nine can be of little or no ſervice) far preferable to every other hitherto diſcovered; yet it will eaſily be acknowledged by every one who is acquainted with what M. DE LUC (m), Sir GEORGE SHUCKBURGH, and Colonel ROY (n), have done upon this ſubject, that the greateſt attention to an infinity of varying circumſtances, as well as the greateſt nicety and exactneſs both in the inſtruments and in repeated obſervations, are neceſſary if we would come at the truth thereby.

Again, the method of taking the difference of heights by the quantity of declivity in rivers requires no attention to the curvature of the globe, an object (as every one knows) infinitely too conſiderable to be neglected in the common method of levelling; as are alſo the great and varying refractions of the viſual rays ſo near to the ſurface of the earth as they muſt be taken in the practice of that method. The quantity of effects and of errors in the viſuals proceeding from this laſt cauſe muſt be very different at different times, as it depends wholly on the greater or leſs denſity, on the greater or leſs quantity of vapours ſuſpended in the loweſt part of the atmo-

(m) See his work on Barometers and Thermometers, in two vol. quarto.

(n) See the learned and curious treatiſes of theſe two gentlemen in the Philoſophical Tranſactions for 1777.

ſphere,

ſphere, the ſtate of which ſeldom remains long the ſame. Now it is no eaſy matter either to determine the quantity of theſe exactly, or to calculate the effects and errors in the viſual rays proceeding therefrom, which yet muſt be done to come at the truth by the common method of levelling; whereas, in the method I propoſe, no ſuch conſiderations are neceſſary, as is evident from the nature of it.

But this is more than enough on a method ſo obvious and eaſy; I ſhall now give a few examples of it, and thereby conclude this eſſay, already perhaps much too long.

83. Suppoſing the length of the Scheld, between Antwerp and Ghent, following all its meanders, to be forty meaſured Engliſh miles, as it is reckoned nearly to be; and ſuppoſing the length of the ſaid Scheld between Ghent and Tournay to be fifty of the ſame miles; and that of the Lys from Ghent to Commines, where it approaches neareſt to the city of Ipres, forty-ſix miles; it is required to know the reſpective differences of elevation between all theſe places.

It may be found above (N° 78. 79.) that the river Scheld, between Ghent and Antwerp, has not above one foot declivity in each mile of its courſe; and that the

Scheld and the Lys, above Ghent, have about one foot declivity in each four thouſand feet of length.

According to this, the ſurface of the Scheld in Ghent is about forty feet higher that it is at Antwerp; and at Tournay it is ſixty-ſix feet higher than at Ghent, and one hundred and ſix feet higher than at Antwerp. So alſo the ſurface of the Lys at Commines is ſixty-one feet higher than at its junction with the Scheld in Ghent, and one hundred and one feet higher than the ſame at Antwerp. From hence it may be deduced, that the Scheld at Tournay is about five feet higher than the Lys at Commines, through twenty-five miles of interjacent country.

84. Suppoſe it be required to find the difference of height between the ſurface of the Lys at Commines and the ſurface of the canal at Ipres which falls into the ſea at Nieuport on the Coaſt of Flanders. The diſtance between Ipres and Commines is nearly ſeven meaſured miles, through which there is no communication by water; but there is one a great way round, which therefore, for the purpoſe required, muſt be followed through all the differences of elevation comprized therein, *viz.*

Deſcent

Deſcent towards the ſea at Nieuport *on the Coaſt of* Flanders.

	Feet.
Total declivity of the Lys from Commines to Ghent, 46 miles, - - - - -	61
Declivity of the canal from Ghent to Bruges, when the ſluices are ſhut, - - -	1
Difference of height of the water in the aforeſaid canal above that in the canal from Bruges to Oſtend, which two communicate together by ſluices, - - - -	8
Declivity from Bruges to Plaſchendahl where the canal of Nieuport joins that of Oſtend, -	6
Total declivity from Plaſchendahl to Nieuport, including the difference of ſurfaces in two intermediate ſluices, - - - -	8
Total declivity, all one way, from Commines to Nieuport, about 95 miles, - -	84

Aſcent

Aſcent from the ſea at Nieuport *to the city of* Ipres.

	Feet.
Difference of height which the river from Nieuport to Ipres has above the canal from Nieuport to Plaſchendahl, taken croſs the harbour of Nieuport, by means of the tides which come up againſt the outer gates of the ſluices next the ſea on each,	3
Declivity of the river Iprelee from Nieuport to Boeſinghe, - - - -	11
Difference of level of the water above and below the ſluice of Boeſinghe, - - -	$22\frac{2}{3}$
Declivity in the canal from Boeſinghe to Ipres, -	$\frac{1}{3}$
Total aſcent, all one way, from Nieuport to Ipres,	37

Now 84−37=47 feet for the difference of height which the ſurface of the Lys at Commines has above the ſurface of the canal at Ipres. I have made uſe of the above example preferably to any others, as it is very complicated, and becauſe the quantities of declivity which I have put down are not arbitrary; and, moreover, becauſe I had the good fortune to find, ſome years after I had taken thoſe meaſures, that I only differed two feet

from what was found by actual levels made from the Lys to Ipres by the French engineers during the time that LEWIS the XIVth was maſter of the country, when there were propoſals for opening a canal from the one to the other.

85. I ſhall venture to carry my conjectures ſtill farther, and grounding them upon the principles laid down above (N° 78. 79.) I ſhall take a general view of the elevations of continents along the courſe of the principal rivers in the known world. I cannot, however, repeat too often, that I give this as a matter of mere conjecture and curioſity. It has not been, nor ever will be, in my power, or in that of any other particular perſon whatſoever, to follow the courſes of all the rivers mentioned in the enſuing table from their mouths to their ſources. All that can poſſibly be done on this head, is to examine the relations of voyagers and geographers concerning each river as far as it is known, and to reduce it by that means within the compaſs of the hydrometrical principles laid down in this eſſay. This is what I have done as far as I could; and therefore, allowing that I make great miſtakes therein, yet I do not think that I merit much blame on that account, as I have done what I was able to do. If I am blame-worthy, it is for having launched out

too far into the regions of conjecture; but as ſuch miſtakes as theſe are no ways prejudicial to my fellow creatures, to whom I wiſh to be uſeful, and as they may give occaſion for others to rectify them, and ſo lead them to a ſubject which otherwiſe, perhaps, they might never have attended to, I ſhall hope for indulgence from all thoſe who wiſh well to humanity and to uſeful knowledge.

86. A table of the elevation of countries above the ſurface of the ſea, at each 100 miles of length up the courſe of the principal rivers in the world, as far as they extend; by computation from the principles laid down in this treatiſe.

Feet of elevation in 100 miles.	Feet of elevation in 200 miles.	Feet of elevation in 300 miles.	Feet of elevation in 400 miles.	Feet of elevation in 500 miles.	Feet of elevation in 600 miles.	Feet of elevation in 700 miles.	Feet of elevation in 800 miles.	Feet of elevation in 900 miles.	Feet of elevation in 1000 miles.	Names of Rivers, And quantities whereby the length of their courſe is to be diminiſhed, to have the diſtance from their mouths in a direct line.
100	210	330	470	630	820	1040	1300	1600	1950	The river Trent, the Meander, and many others of the ſame kind, which are ſeldom of great extent: in theſe one-third of the courſe may be allowed for its deviations from a right line.
150	310	480	670	880	1120	1400	1770	2160	2620	The river Thames; the Seine and the Somme in France; the Nile and the Niger in Africa; the river St. Laurence, the Oroonoko, the river of Amazons, the rivers of Paraguay: in theſe about one-fourth of the length of courſe may be allowed for turns and windings in it.
220	450	700	980	1290	1640	2040	2500	3030	3630	The Severn; the Loire and Garonne in France; the Tagus, Guadiana and Guadalquivir in Spain; the Po and the Tyber in Italy; the Meuſe, Rhine and Elbe in Germany; the Weixel, Neiſter, Bog and Nieper in Poland; the Don and Dwina in Ruſſia; the Amur in Tartary; the Hoang-ho-keou and Yang-tſe Kiang-keou in China; the rivers of Cambodia, Ava and Ganges in India; the Euphrates; the Zaire in Congo; the Miſſiſippi: in theſe may be allowed about one-fifth of the length of the courſe for turns and windings in it.
300	650	1050	1520	2070	2720	3500	4440	5570	6920	The Rhône in France; the Ebro and Douro in Spain; the Danube; the Wolga; the Irtiſch, Oby, Jeneſca and Lena in Siberia; the Malmiſtra in Cilicia; the Tigris; the Indus. The courſe of theſe rapid rivers is uſually very ſtraight, and there cannot be above one-ſixth of the length thereof allowed for deviations from a right line.

XXXVIII. *Extract of two Meteorological Journals of the Weather, observed at* Nain, *in* 57° *North Latitude, and at* Okak, *in* 57° 30′, *North Latitude, both on the Coaſt of* Labradore. *Communicated by Mr.* De la Trobe *to the Preſident, and by him to the Society.*

Read May 20, 1779.

THE thermometer, of FAHRENHEIT's ſcale, was obſerved at eight in the morning, at noon, at four in the afternoon, and at eight in the evening.

The barometer, whoſe ſcale is French meaſure, was obſerved at eight in the morning, and at eight in the evening.

Months.

Months.	Thermometer at Nain.			Barometer at Nain.			Thermometer at Okak.		
1777	Higheſt	Loweſt.	Mean.	Higheſt	Loweſt	Mean.	Higheſt	Loweſt.	Mean.
	°	°	°				°	°	°
Auguſt	76,0	36,0	52,3	28,3	27,6½	27,10,4	74,0	36,0	52,0
Septemb.	72,0	27,0	43,6	28,5½	27,6½	28,0,4	68,0	21,0	43,9
October	51,0	12,0	30,2	28,7½	26,11½	28,0,1	48,0	13,0	30,0
Novemb.	41,0	— 8,0	15,4	28,9	26,9	28,0,0	39,0	— 5,0	17,5
December	31,0	— 26,0	10,3	28,5½	26,3	28,3,0	32,0	— 28,0	4,2
1778									
January	18,0	— 30,0	— 12,2	28,5	27,1½	28,4,5	16,0	— 27,0	— 11,0
February	32,0	— 27,0	— 10,7	28,6	27,3	28,7,6	34.0	— 25,0	— 9,2
March	27,0	— 25,0	4,3	28,10½	27,2½	27,4,0	32,0	— 18,0	6,5
April	49,0	— 8,0	27,1	28,6½	27,6	28,4,3	53,0	— 10,0	28,5
May	68,0	29,0	39,0	28,5½	27,7	28,7,5	65,0	27,0	37,5
June	77,0	33,0	45,0	28,6	27,4	28,0,0	73,0	31,0	45,7
July	82,0	34,0	51,0	28,3½	27,8½	28,0,1	81,0	36,0	51,3
Auguſt	74,0	40,0	52,5	28,5	27,5½	28,0,0	72,0	41,0	52,0
Septemb.	69,0	34,0	43,5	28,0	27,10	28,2,0	53,0	32,0	43,0

IMPROVEMENTS

IN

ELECTRICITY.

By JOHN INGENHOUSZ, F.R.S.

BODY PHYSICIAN TO THEIR IMPERIAL MAJESTIES.

Who was nominated by the Preſident and Council to proſecute Diſcoveries in Natural Hiſtory, purſuant to the Will of the late HENRY BAKER, Eſq. F. R. S.

Read at the ROYAL SOCIETY, June 3, 1779.

TRACTATUS

IMPROVEMENTS IN ELECTRICITY.

BEING appointed to deliver the annual diſſertation on ſome philoſophical ſubject, inſtituted by our worthy brother the late Mr. HENRY BAKER, I will endeavour to explain ſome contrivances in electricity which I invented a good while ago, one of which has been much employed, and has undergone ſeveral material improvements ſince I firſt thought of it.

IT is now about fifteen years ago ſince I began to make uſe of flat glaſſes inſtead of globes or cylinders, to excite electricity. Finding that a greater quantity of electricity could be excited upon a flat piece of glaſs, when rubbed on both ſurfaces, than when it was only expoſed to friction on one ſide; I thought it would be an advantage to ſubſtitute a round plate, or disk, of glaſs, to a globe

or cylinder. I alſo thought another material advantage might be derived from a plate of glaſs, as the form of it admits of placing cuſhions or rubbers upon different parts of it, and taking the electricity, excited by theſe rubbers, from the interſtices between them, which cannot conveniently be done when a globe or cylinder is uſed. The only inconvenience which I at firſt conceived would enſue from it was, that the center upon which the plate was to be fixed and whirled round, would always be too near the rubbers (unleſs theſe were very ſhort, or the plate of a conſiderable ſize) and that theſe would throw the electricity, collected on the ſurface of the glaſs, upon that very center. In order to obviate this difficulty, I propoſed to make the center likewiſe of glaſs, or ſome other non-conducting ſubſtance, as, for inſtance, baked wood.

I began firſt by making uſe of one of thoſe glaſs ſtands, which they call a waiter, and which has a glaſs ſupport fixed at right angles to its center. I whirled the waiter round as well as I could, rubbing it ſometimes on one ſide, ſometimes on both. In this imperfect ſtate I ſhewed it to Dr. FRANKLIN, who approved much of the ſcheme, and adviſed me to purſue it. Soon after, I ſhewed it to ſeveral of my acquaintance, and in a ſhort time I found ſuch machines ready made at Mr. RAMSDEN's and ſome

other mathematical inſtrument-makers. Since that time great uſe has been made of theſe electrical plate machines throughout Europe, as they were thought by many to be more powerful in a little compaſs than thoſe with globes and cylinders.

In my travels through different countries, I now and then met with conſiderable improvements made in them. Abbé FONTANA had contrived one for the cabinet of the great Duke of Tuſcany, which conſiſted of two plates, eighteen inches diameter, fixed to the ſame center, and each rubbed on both ſides on two oppoſite places. The electrical fire excited on theſe joint plates, when forced upon a conductor divided in two branches to receive it, was very powerful; ſo much ſo, that the conductor, being unable to contain the whole, threw it back upon the braſs center; from which it paſſed to the hand of the operator, and gave him a very diſagreeable ſhock.

Mr. CUTHBERTSON, an ingenious mathematical inſtrument-maker at Amſterdam, contrived an apparatus with a double plate, by which all the electrical fire collected by the eight cuſhions was forced upon the conductor, ſo that none of it could be thrown back upon the center, though made of braſs. His contrivance conſiſted in placing between the glaſs plates a ſtrong glaſs ring, about two inches in diameter, ſo that the braſs

center paſſed through the middle of it. This ring was ſtuck to the plates with ſealing wax or ſome other non-conducting cement; and the ſpace between the center and the ring was carefully filled with the ſame non-conducting ſubſtance. The conductor had two branches, each of which was placed between the two glaſſes, reaching very near to the glaſs ring. By this method all, or almoſt all, the electricity excited by the eight cuſhions, was forced to paſs upon the conductor, there being no way to reach the braſs center, between which and the conductor all communication was cut off by the above mentioned glaſs ring being filled up with a non-conducting cement. The power of ſuch a machine, notwithſtanding that the plates were not above fifteen inches diameter, was very aſtoniſhing. I ſaw one made in London with this improvement (the glaſs plates being eighteen inches diameter) by which a coated jar of two quarts was fully charged in leſs than five ſeconds.

Mr. C. CUYPERS, an ingenious electrician at Delft, has not a little contributed to the improvement of theſe machines, by making them leſs liable to be affected by damp weather. This gentleman, conſidering that all glaſſes are not equally fit for electricity, and that J. H. WARTZ, and after him Profeſſor MUSSCHENBROEK, were of opinion, that glaſs, in the compoſition of which there enters

 a great

a great deal of alkaline ſalt, is very apt to attract moiſture from the air, and therefore leſs proper for electricity (which defect they thought might be corrected by expoſing it to a violent and continued heat), took a proper advantage of this knowledge in the improvement of the machines with flat glaſſes. He found, that glaſſes, which have been many years expoſed to the warm air of a room, very old looking glaſſes for inſtance, become by this means much harder, ſo as better to reſiſt the force of a file; and are then much better for electrical machines: and that ſuch glaſſes become ſtill incomparably better, if they are expoſed to a conſiderable degree of heat during ſome months: the heat forcing out of the glaſs (at leaſt out of its ſurface) the alkaline ſalt, not vitrified, which is to be found upon it, and may be known by the taſte.

In December 1777 I ſaw one of theſe double-plated machines at Mr. CUYPER's houſe, and found it do admirably well, though the weather was at the time very damp, and the machine kept in a room in which there never was any fire made, and though the cuſhions had no amalgama upon them: they were made of yellow Turkiſh leather, ſtuffed with fine ſhavings of cork, rammed in them; and had been preſſed to give them an equal ſmooth ſurface.

The

The ſame gentleman found glaſſes, prepared in the above mentioned way, far ſuperior in ſtrength to a cake of roſin uſed in the electrophorus. He publiſhed his method in a pamphlet, intitled, *Expoſé d'une Methode, par la quelle on rend les Diſques de Verre deſtinés à des Machines Electriques capables d'exciter l'Electricité dans une Air humide, ſuivi d'une Maniere de faire de très bons couſſins pour frotter les Verres des Machines electriques, et de la Deſcription d'un Electrophore perpetuel plus parfait que ceux dont on s'eſt ſervi juſq' ici.* A la Haye, 1778.

Thoſe who make uſe of plate machines ſhould carefully avoid putting the apparatus near the fire, for the purpoſe of drying or warming it; becauſe the ſudden expanſion of the glaſs by the heat cannot ſo quickly propagate itſelf through its whole extent; for the center being commonly ſqueezed between two flat ſhives of braſs, with a piece of leather between the metal and the glaſs, does not acquire a ſimilar degree of heat at the ſame time as the reſt, and cannot ſo eaſily expand; and therefore the plate is in great danger of breaking. If, in conſequence of ſuch a blunder, a flaw ſhould happen, its progreſs might be ſtopped by drilling a round hole at the extremity of the flaw. Theſe flat glaſſes may very ſafely be rubbed with a dry warm cloth.

As

As the quantity of electricity excited upon glaſs is nearly in the proportion of the ſurface expoſed to friction; and as glaſſes of a great ſize are very precious, and liable to accidents, I conceived, that inſtead of a disk of flat glaſs one might ſubſtitute one of paſte-board, thoroughly imbibed with copal or amber varniſh.

To try how this would anſwer, about ſeven years ago I ordered three paſte-board disks to be made, of four feet in diameter, the diſtance of ſix inches from the center being the fitteſt to give the whole a proper ſupport in whirling it round. When theſe disks were thoroughly dried and heated, I poured upon them a varniſh made of amber diſſolved in linſeed oil. After they had taken in as much of the varniſh as they could imbibe, I covered them with a thick coat of the ſame varniſh, and dried them by the heat of a German ſtove.

When the varniſh was very hard, I found, that even a ſlight friction with a cat's skin or hare's skin excited a ſtrong electricity upon them.

I then made a frame to place them in, and to whirl them round; which frame was ſo contrived, that it could contain about twelve ſuch disks, whirling all round on the ſame center. It conſiſted of two ſquare pillars of wood, about five feet high, and three inches broad; joined together at top and bottom by a tranſverſe piece of

of wood. In the middle of the two pillars was a hole, about an inch and an half diameter, fitted to receive a wooden axis, which could be placed in, and taken out, at pleaſure. Upon this axis were to be ſtuck the paſte-board disks; and a flat board, three inches broad, covered on both ſides with flannel, and over this with a hare's skin, was to be placed between each paſte-board. The two ſquare pillars were alſo to be wrapped up firſt with flannel, and over that with hares skin.

The flat boards, to be placed between the paſte-board disks, had each a notch in the center, to give room for the axis to turn round freely. Theſe flat boards could be brought as near one another as was required by two wooden male ſcrews, placed at the upper and lower end of the frame, which reached from one ſquare pillar to the other; which ſcrews were to receive a notch cut out at the upper and under end of each flat board, in order to keep them ſteady in their vertical ſituation. A female ſcrew, turning upon theſe horizontal male ſcrews, was placed between each of the flat boards at their upper and under extremities, and ſerved to bring each of the disks as near in contact with the hares skins as was required to receive a proper friction.

The three paſte-boards were fixed in the frame, and whirled round. The electricity excited was ſo ſtrong

that

that I took ſparks between one and two feet long from the front ſurface of the firſt disk by approaching my knuckle to it. I then applied a tin conductor to it, about ſix feet long and ſix inches diameter, divided into two branches, the extremities of which were furniſhed with a thick ſilver lace fringe inſtead of points. The ſparks from this conductor were about four or five inches long, appeared to be very thick, were very brilliant, and ſo ſtrong, that I did not chuſe to receive many of them; nor did thoſe who came to ſee the machine care to receive more than one. As theſe ſparks ſucceeded one another at ſhort intervals, I think they would have been much longer if every thing had been adapted for that purpoſe; as I ſaw afterwards done at Mr. NAIRNE's, who contrived to obtain ſparks of twenty inches long and upwards from a large glaſs cylinder.

I conſidered this paſte-board machine rather as a rough ſketch of an apparatus, by which I conceived the hope of obtaining an electrical power of almoſt any degree required, than as a compleat machine. My intention was to find out a contrivance by which a very great quantity of electrical fire might be collected without great expence and without much danger of breaking the apparatus, which two articles cannot be avoided when you make uſe of uncommon ſize glaſs cylinders or disks.

I ſaw, two years ago, at the Duke of CHAULNES at Paris, an apparatus which had a plate glaſs five feet diameter. This alone coſt him eight hundred French livres.

As I had not adapted the tin conductor to receive the electricity from the three disks, but only from the front disk, I cannot tell whether the force of electricity would have been proportionably ſtronger if I had made ſome metallic communication between each of the disks.

I found, that the apparatus, as it was conſtructed, could not eaſily have admitted more than three ſuch large disks; for the twelve ſurfaces expoſed to friction being each a foot and an half long, and above three inches broad, made ſo much reſiſtance to the working of the machine, that it required a ſtrong arm to work it.

Being ſatisfied with having found that by this, or a contrivance of the ſame kind, a much greater power could be excited than by the common glaſs apparatuſes; I did not chuſe to put myſelf to more trouble or expence to increaſe the ſtrength of its frame, or the number of disks.

I muſt obſerve, that ſuch a machine may be kept in good order in a heated room; but that it will ſoon loſe its force in countries where it is not the cuſtom to heat rooms as the Germans do. My paſte-board disks kept

very good during all the time I left them in a room conſtantly warmed, which was about two months; but, when they were placed in a cold room, they ſoon loſt their power, having probably attracted moiſture from the air. I cannot be ſure that the varniſh I got made for the purpoſe was of the beſt kind: I have reaſon to ſuſpect the contrary, and therefore I ſhould think that much better might be obtained in London.

Such paſte-boards might be poſſibly preſerved from attracting moiſture, by keeping them ſhut up in a box made on purpoſe, lined within with tinfoil. The moiſture might alſo be expelled again by placing them a good while in a heated room, or upon a baker's oven.

It ſeems, beſides, not improbable, that a kind of paſte-board might be made by ſticking together the lamina of paper (firſt thoroughly dried) with a good oil varniſh inſtead of common paſte, as this laſt never can be deprived of its watery particles without loſing its coheſive quality. A good kind of paſte-board might likewiſe be contrived by ſticking together ſilk cloth inſtead of paper.

I contrived a plated machine, the disk of which was made of baked wood and boiled in linſeed oil; but it did not anſwer near ſo well as the paſte-board disks.

I found the paſte-board disks not much leſs ſuſceptible of electricity before I had varniſhed them than they were afterwards; but they loſt their force again in a few minutes, and did not recover it till they were dried and heated again.

It is well known, that writing and brown packing paper, when warmed, may acquire a conſiderable electrical power by being rubbed with hares skin, a piece of wood, ivory, nay even (as I found by experience) with a metallic body.

As it ſeems to be a general law of nature, that all reſinous bodies, excited either with a poſitive or a negative electricity, are more tenacious of keeping it, or ſeem to part with it, as it were, with more reluctance than glaſs (as I have demonſtrated in a paper, read laſt year before the Royal Society); it is adviſeable, that the conductor of ſuch a paper machine be not furniſhed with metallic points, which being neceſſarily kept at a diſtance will not take away all the electricity; but that ſome flexible, conducting ſubſtance, as ſilver or braſs lace fringes, communicating with the conductor, be in cloſe contact with the excited ſurface.

As woollen cloth or Mancheſter cotton velvet, and ſuch like ſubſtances, excite a good deal of electricity upon dried paper and reſinous ſubſtances, and do not wear out

ſo

ſo ſoon as hares skin, it might, perhaps, be found better to ſubſtitute them for hares skin.

I have alſo excited a very conſiderable electrical force on ſtrong ſilk velvet, tied upon the circumference of two wooden disks, two feet in diameter, and diſtant about three feet from one another, fixed upon a wooden axis. The velvet was ſupported by a ſtrong ſilk cloth tied under it, in order to give it more ſtrength and ſteadineſs. This machine had the appearance of a drum, and was whirled round, as is uſually done with glaſs cylinders. The rubber was a cuſhion covered with hares skin.

As ſilk cloth, and more particularly oiled ſilk, very eaſily receives a ſtrong electricity, I make no doubt but a good uſe might be made of them, by expoſing a great ſurface of them (which may be as large as one pleaſes) to friction. I have attempted more than one method of conſtructing ſuch a machine; but as I tried it only in ſmall, I have not purſued the object far enough, and therefore, I think, I have no right to throw out hints unſupported by experience.

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16. 15. *for* an ounce and a half, *read* a drachm and a half.
24. 5. *for* 5 ounces 36 grains, *read* 5 drachms 36 grains.
27. 4. *for* Zoolite, *read* Zeolite.
30. 19. *for* tin, *read* fire.
122. 10. *for* percurſio, *read* percuſſio.
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131. 14. *for* it may obſerved, *read* it may be obſerved.
182. 2. *for* Stephens, *read* Stevens.
243. laſt line of the Engliſh, *for* pea, *read* pen.

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364. column the 4th, *for* Mr. de Luc's thermometer, *read* Mr. de Luc's rule.
373. column the 6th, line the laſt, *for* 96, *read* 98.
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380. 15. *for* open on both ſides, *read* open at both ends.
540. *for* working-needle, *read* ſewing-needle, *paſſim*.

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684. *for* Eric Proſperin, *read* Proſperin Eric.
685. 22. *for* piſtol to make it, *read* piſtol with it.
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